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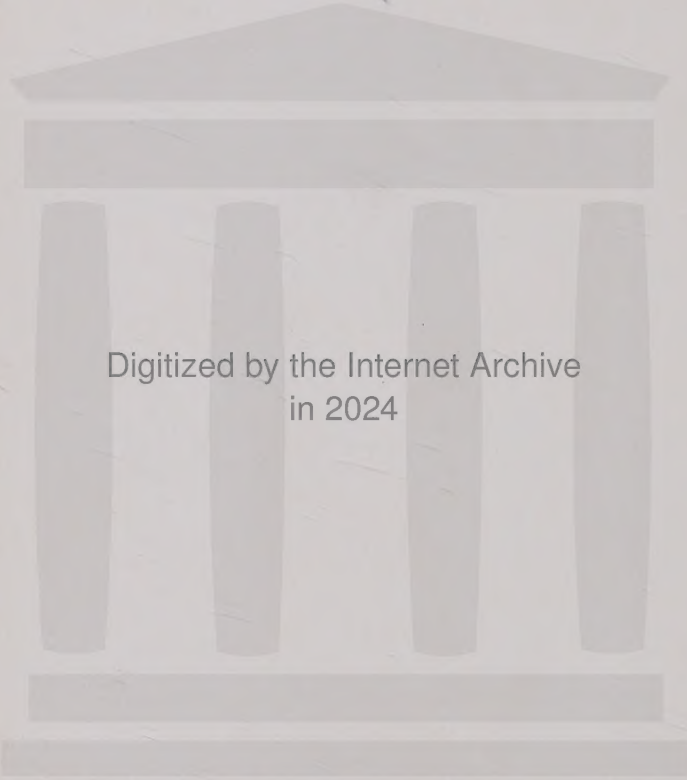
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NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

BIOGRAPHICAL MEMOIRS

VOLUME XIV—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

SIDNEY IRVING SMITH

1843-1926

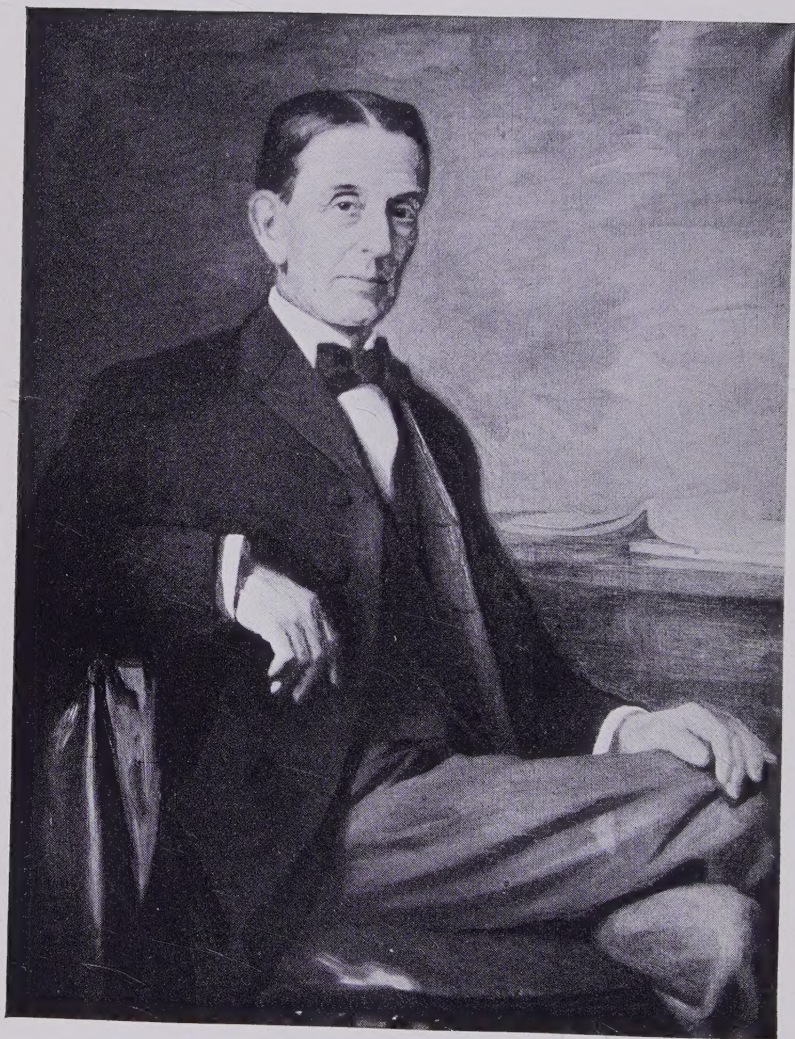
BY

WESLEY R. COE

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1929

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Sidney J. Smith.

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SIDNEY IRVING SMITH

1843-1926

BY WESLEY R. COE

The life of Sidney Irving Smith, professor-emeritus of comparative anatomy at Yale University, well known to a previous generation of zoologists for his studies on the systematics and life histories of the crustacea, was brought to a close on May 6, 1926, at the age of eighty-three years.

Professor Smith was born in Norway, Maine, February 18, 1843. Both his parents were of early New England ancestry and of families with an unusual proportion of prominent men. As a boy he was greatly interested in natural history and he early came under the influence of his later brother-in-law, Addison E. Verrill, at that time a pupil of Louis Agassiz at Harvard. In 1864, when Verrill received his appointment as the first professor of zoology at Yale University, young Smith came with him as a student in the Sheffield Scientific School.

Before coming to Yale, Smith had already made an extensive collection of the insects of his native State. So carefully prepared and so accurately labelled was this collection that it was purchased by Louis Agassiz for the Museum of Comparative Zoology at Harvard College. Finding no similar collection at Yale, Smith spent parts of his vacations during many following years in making a new collection for Yale's Museum, and the Peabody Museum of Natural History at Yale now contains thousands of beautifully prepared specimens which testify to his zeal and skill as a collector and to his accuracy of identification.

In his senior undergraduate year at Yale, Smith became a charter member of the Berzelius Society, and for his essay on the "Geographical Distribution of Animals" was awarded the first of the prizes which have been given by that society. This paper was later published in the *American Naturalist* (1868).

On graduating from Yale with the degree of Ph. B., in 1867, he was appointed assistant in zoology in the Sheffield Scientific School, and with this institution he remained connected for

thirty-nine years—until his retirement from active service in 1906. He was promoted to the professorship in comparative anatomy in 1875.

During all his summer vacations Smith was active in field work, gathering at such times material for his studies during the college year, for he was an indefatigable worker, for many years a bachelor—his teaching and his research receiving his entire attention.

His first official appointment outside the university was as zoologist on the United States Lake Survey in 1871. At this time he participated in a zoological reconnaissance of the deeper parts of Lake Superior. Extensive collections were made and a number of new species of invertebrates discovered. The results of this work were published by him in several papers included in the "Report of the Commissioner of Fish and Fisheries" for 1872, part 2. One of these (1874) gives a summary of the invertebrate fauna of the lake, another (1874) discusses the food of fresh-water fishes, a third (1874) treats of the crustacean parasites of fresh-water fishes, including descriptions of new species, while a fourth (1874) consists of a synopsis of the higher fresh-water crustacea of the United States. Briefer papers on the same topics appeared in the *American Journal of Science* and in the *Canadian Naturalist*.

Portions of each summer from 1864 to 1870 were spent in assisting Professor Verrill on dredging expeditions in Long Island Sound and in the Bay of Fundy. Smith's attention was thus directed to the then almost virgin field of marine invertebrates, particularly the crustacea, and to this branch of zoology he devoted most of his life work, soon becoming the leading authority on the marine crustacea of the western Atlantic. In prosecution of these studies he joined the United States Coast Survey for work on St. George's Bank in 1872, and he was a regular member of the scientific staff of the U. S. Fish Commission during each summer for the ensuing fifteen years.

During all these years he had charge of the vast collections of crustacea taken in the dredgings, and was thus in a position to make known to science the great number of new and

interesting types which they contained. His extraordinarily careful and complete descriptions of these new forms have needed no later revisions, and have been of much value to later students in their interpretation of the relationships and evolutionary history of the crustacea.

Most of the papers in which these new forms were described were published in the *American Journal of Science*, in the "Proceedings of the U. S. National Museum," in the reports of the U. S. Commission of Fish and Fisheries, in the "Transactions of the Connecticut Academy of Arts and Sciences," and in the "Bulletin of the Museum of Comparative Zoology." All of them are models of accuracy, faithfulness to details, skill in illustration, and discrimination in the interpretation of specific and generic relationships.

Most of the type specimens of the new species are to be found in the United States National Museum. With characteristic generosity Smith presented all his cotypes and all his extensive series of the various species in the collections to the Peabody Museum of Natural History at Yale and here they are available to other workers in that field of zoology.

Although Professor Smith's systematic work on the fresh-water and marine crustacea entitles him to a position in the front rank of American systematic zoologists, his studies on the life histories of the crustacea proved of more general interest. He was the first to interpret correctly the successive stages in the larval life of the American lobster (1872, 1873); and his descriptions of the complicated metamorphoses which occur during the early life of other crustaceans, particularly of Ocypoda (1873), Hippa (1877), Pinnixa (1880), and Panopeus (1883), have found a wide application in interpretation of the relationships of the various groups.

For several years prior to 1874 he assisted Professor Verrill in the preparation of the classic "Report on the Invertebrate Animals of Vineyard Sound"; an ecological study that had no parallel in America for more than forty years. Professor Smith prepared all the material relating to the crustacea and revised other parts of this widely used book.

While in his later years his scientific research was devoted entirely to the crustacea, Professor Smith was a broadly trained naturalist of the old school, with a keen eye and an inquiring mind. An eager disciple of the then controversial Darwinian theory of evolution, he sought for verification of this hypothesis in all he saw about him. His first paper (1864) was on the fertilization of orchids and his second (1865) on a new species of moth. He was always much interested in insects, gathering extensive collections and reporting on the new and little known species which he found. In 1872-3 he served as State Entomologist of Connecticut and contributed to the reports of the Secretary of the Board of Agriculture for those years.

Smith was one of the founders of the Marine Biological Laboratory at Woods Hole, and one of the early members of its board of trustees. His last scientific work was the revision of the definitions in comparative anatomy for "Webster's International Dictionary" in 1890. Then in the prime of life he accepted ever-increasing administrative duties in connection with the Sheffield Scientific School, and allowed himself to become burdened with committees to which he devoted himself with all the zeal which he had previously shown for science. In the sixteen years prior to his retirement he contributed much to the upbuilding of the Sheffield Scientific School, but this was done, as has been the case with so many other scientists who have accepted executive duties in middle life, at the sacrifice of his scientific career.

He was married in 1882, then nearly forty years of age. His wife, Eugenia Pocahontas Barber, died in 1916. There were no children.

Professor Smith never had the appearance of very robust health, and although his long life testifies that he must have had a sturdy constitution he found it necessary to guard his health carefully. He was extraordinarily modest, quiet and retiring, diffident, and always reluctant to appear in any conspicuous position. And yet, in private company and in his home, he was not only charming in his kindly manners but

extremely entertaining. Only rarely did he and Mrs. Smith appear at public functions, the simplicity of their home life, with an abundance of books, being quite sufficient for their entertainment. He followed the example of his brother-in-law, Prof. A. E. Verrill, in absolutely refusing tobacco and all of the forms of alcohol.

In the classroom and particularly in the laboratory Professor Smith endeared himself to even the most indolent of his students. Demanding of himself the greatest exactness in his work, he expected it of others, and he was justly considered one of Yale's best teachers because of his ability to inspire in his students a real desire for a similar standard of excellence. No teacher at Yale in his day was held in greater affection or esteem. Even the penalties imposed by the discipline committee, of which he was long a member, were accepted without resentment by the students because of the sympathetic manner in which they were administered.

As blindness was approaching some twenty years before his death, Professor Smith continued his daily chores about the house and by means of an ingenious arrangement of sticks and strings he was able to continue work in his garden even when totally blind. He planted and gathered his flowering bulbs each season and sowed his vegetable seeds in symmetrical rows and harvested the crops, guided entirely by the delicacy of the senses which became more acute in compensation for the loss of sight.

His blindness was due to hereditary glaucoma, and it is sad to record that in his later years he was also afflicted with a cancerous condition of the throat which caused him intermittent paroxysms of severe coughing that nothing seemed to relieve. Even the combination of these terrible afflictions failed to alter his serene and cheerful disposition, and he bore them to the end of his life with uncomplaining fortitude.

Professor Chittenden says of him in the "History of the Sheffield Scientific School" (Yale University Press): "Professor Sidney I. Smith was a born naturalist, in the older sense of that term, having an ardent love of nature and a great

passion for studying her different forms of life. He was endowed with great ability, keen discrimination, and a love of truth which permitted no obstacle to stand in the way of a correct description of the types of life he was interested in."

He was among the first zoologists in the country to put in practice Huxley's plan of intensive laboratory work in biology, comparative anatomy and embryology as one of the fundamental studies for pre-medical students.

He was elected to membership in the National Academy of Science in 1884, and was one of the charter members of several of our biological societies. Yale conferred on him the honorary degree of M. A. in 1887.

His bibliography includes about seventy titles. Some of these papers are brief, others more extensive, but all are characterized by such thoroughness and faithfulness to detail that his work will stand the test of future generations, and will not have to be repeated.

A biographical notice of the life and work of Professor Smith was published by A. E. Verril in *Science*, vol. 64, pp. 57-58, 1926, and one by W. R. Coe in the *American Journal of Science*, vol. 12, pp. 463-466, 1926.

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Yale University

PART I. BIOGRAPHY

ADDISON EMERY VERRILL, for forty-three years professor of zoology at Yale University, whose death occurred at Santa Barbara, California, December 10, 1926, had been a member of this Academy for more than half a century, having been elected to membership in 1872. He was within about two months of having completed his eighty-eighth year, for he was born at Greenwood, Maine, February 9, 1839.

Professor Verrill was of old New England stock, the second son of George Washington and Lucy (Hillborn) Verrill. The Verrill line descended from an ancestor who settled in Gloucester, Mass., about 1720, and the Hillborn line from a Quaker who settled in Pennsylvania previous to 1688. The names of Cordwell, Garland, Stevens, Shreeve, Waterhouse, and of other substantial pre-revolutionary families make up his American ancestry.

In 1864, when Louis Agassiz, at Harvard, and Joseph Leidy, at Philadelphia, and a few others had developed the study of animal life to such an extent that zoology had begun to be recognized in America as one of the sciences, Yale College called upon Agassiz for a young man who would bring the new science to New Haven. Fortunately Agassiz then had as assistant in the Museum of Comparative Zoology the man who served as professor of zoology at Yale until he was retired as professor-emeritus in 1907. When appointed at Yale, Verrill was only 25 years of age, having received his bachelor's degree from Harvard two years previously.

It is perhaps needless to state that a naturalist of such exceptional ability in his manhood exhibited similar talents in his boyhood. Before his thirteenth year he had learned to recognize the varieties of rocks and minerals to be found in his native town and had assembled a collection of considerable

size. The following year he turned his attention to the study and collection of wild flowers. He soon had an herbarium of several hundred species, each of which he could recognize and name almost instantly throughout the rest of his life. At seventeen he had acquired a collection of the local shells, insects, amphibia, reptiles, birds and mammals, making the identification, when possible, with the aid of such few books as were available to him, and noting especially the kinds which were different from any described in his books. In this way he laid a broad foundation for the taxonomic studies which were to constitute such a large part of his life work. By these boyhood activities he trained his powers of observation and developed a keenness in logical deduction which were destined to serve him for more than seventy years in the interpretation of the significance of the facts which came under his observation.

Shortly after his fourteenth birthday young Verrill's family moved to the not distant town of Norway, Maine, and the boy entered the local academy known as the Norway Liberal Institute. Here he pursued such studies as would prepare him for entrance to Harvard University and thereby to the laboratory of Louis Agassiz, but his real education was obtained in the woods and in the fields and almost entirely without the aid of books.

He arrived in Cambridge in May, 1859, and soon realized his ambition of working under the direction of the great Agassiz. He often referred to his first experience in the laboratory of that distinguished zoologist. Instead of listening to lectures and studying books, he was asked what field of zoology most appealed to him. On replying that he was most interested in birds, Agassiz directed him to make a study of the goose. After some weeks, when young Verrill had completed what seemed to him an absolutely exhaustive study of every part of the bird's external and internal anatomy, Agassiz genially pointed out to him the incompleteness of his investigation and gave him directions for several months' additional work before the subject was deemed sufficiently mastered. A new topic of study was then introduced.

In this connection the embryology of birds was studied and Agassiz suggested that the embryos of the diving sea birds might throw some light on problems connected with the relationships and evolution of that group. In order to secure these embryos, Verrill went to the island of Grand Menan, in the Bay of Fundy, and at the end of the summer (1859) brought back to the Museum of Comparative Zoology about 1200 embryos in addition to numerous skeletons of the birds and some skeletons of fishes.

The following summer he joined two fellow students, Alpheus Hyatt and N. S. Shaler, in a study of the marine invertebrates on the coast of Maine, and this was Verrill's introduction to those groups of animals to which he was destined to devote the major part of his long and active life.

Each of these three young explorers later became distinguished, two of them being elected to membership in the National Academy of Sciences, and their close association during these and other summer months must have been of mutual benefit, for each represented a distinct type of mental activity. Hyatt was essentially a philosopher, ever lost in speculations concerning the significance of the adaptations of the animals which he discovered. Hyatt's friendship was continued through life, and in his honor Verrill named his second son, Alpheus Hyatt Verrill, who has become well known as the author of many books on popular science.

Shaler, on the other hand, was gifted in generalizations and in the popularization of science, having so rare a personal charm and such facility in the elucidation of scientific truths that he later became Harvard's most popular and beloved science teacher.

Verrill, in contrast, was the painstaking, hard-working investigator to whom no detail seemed trivial enough to be neglected; one who was willing to devote himself to the task of discovering the most minute of such morphological characteristics as might distinguish two closely allied species of animals, no matter how great the personal sacrifice. Moreover, he could just as patiently struggle with the ambiguous writings of the earlier naturalists, sometimes straightening out a tangled synonymy which would have discouraged almost anyone else.

In the summer of 1861, Hyatt, Shaler and Verrill set forth on a second expedition; this time to investigate the little-known island of Anticosti, in the Gulf of St. Lawrence, and portions of the coast of Labrador. A small fishing schooner, *The Inlet*, was chartered and Captain Small secured as master. The three students and a friend made up the entire crew. The expedition was a complete success, the various members of the party later publishing accounts of their discoveries in regard to the geology, palaeontology, botany and zoology of the regions visited. Verrill wrote on the mammals, birds and plants. (See papers No. 2, 3, in the bibliography.)

In his junior undergraduate year, 1860, he was appointed by Agassiz as assistant in the Museum of Comparative Zoology. He retained this position during his senior year and for the two years following his graduation (1862).

The versatility of his boyhood studies in natural history now began to bear fruit, for in the years 1862 and 1863 he published no less than 22 papers, of which two were on minerals, one on plants, three on corals and their allies, seven on birds, four on mammals, three on amphibians and the others on general natural history. Most of these were brief taxonomic papers of local species, but one of them, on the revision of the Polypi of the eastern coast of the United States (No. 140), showed a remarkable comprehension of the principles of taxonomy and formulated a system of classification which has continued in use to the present day.

Each summer from 1863 to 1870 Verrill continued his studies on the marine invertebrates found along the coast of Maine, and extended his collections by means of dredgings off the coast and in the Bay of Fundy. During the college year he worked up these collections and recorded the distribution of the various species, giving special attention to those that were new to science. Of the latter he published full and accurate diagnostic descriptions, for he had an almost incredible faculty for discriminating between trivial individual variations and true specific distinctions.

In these years also, he was fortunate in securing for the Yale Museum extensive collections of marine invertebrates from

both the east and west coasts of Central and South America. These, of course, contained many new species of coelenterates and echinoderms, and formed the basis of numerous rather brief publications as well as material for his later, more comprehensive monographs.

On his arrival at Yale, Professor Verrill immediately set about the development of a zoological museum out of the old "Natural History Cabinet" which the Yale Natural History Society had long before established in one of the small buildings of the University, but cramped quarters gave little opportunity for exhibiting his collections. The building of the Peabody Museum of Natural History in 1875 offered space for more extensive exhibits and he, with Professor Sidney I. Smith and several assistants, prepared and arranged a zoological collection for public exhibition which for many years compared favorably with that of any college museum in the country. Professor Verrill remained in charge of the zoological collections until 1910, by which time they had increased until they contained more extensive series of marine invertebrates than were to be found in any American museum with the exception of the United States National Museum and possibly also the Museum of Comparative Zoology at Harvard.

The building which held these collections was removed in 1916 and the zoological material was later installed in the new Peabody Museum of Natural History, where it is now available for reference and further comparative studies.

Shortly after coming to Yale, Verrill fitted up a dredge operated from a sailboat, and on many Saturdays and Sundays during term time, with the assistance of several of his students, made a very thorough zoological survey of New Haven Harbor and adjacent portions of Long Island Sound. To the end of his life he retained ownership of one of the Thimble Islands, about fourteen miles east of New Haven. Here, for nearly forty years, he spent the summer months with his family, offering cordial hospitality to his students and keeping yearly records of such changes as occurred in the invertebrate fauna of the adjacent shores.

In 1871, when the United States Fish Commission inaugu-

rated a comprehensive survey of the waters off the coast of New England with the object of securing information regarding the environment of the commercial fisheries, Verrill was selected as the logical person to take charge of the scientific investigations. And from that time until 1887 there came into his hands an almost continual stream of material dredged from the ocean bottom and containing a great number of forms of animal life quite different from any that had been previously known. These were busy years, with numerous publications describing the new things that were discovered, and before the work was discontinued the Peabody Museum at Yale had become the repository of hundreds of thousands of specimens, among them being several hundred species previously unknown.

Instead of distributing this mass of material to specialists as is the rule at the present day, Verrill covered all the groups of invertebrates except the protozoa. He published upwards of a hundred papers on these collections of the Fish Commission, describing the hundred or more new species which came into his hands and supplementing the diagnoses of those which had been previously reported. Among them were representatives of previously unknown orders, families and genera.

In connection with these surveys the sea-bottoms off the coast, from Cape Hatteras to Newfoundland and out to depths of over 4000 meters beneath the Gulf Stream, were explored by means of the dredge. Several new types of collecting apparatus, such as the cradle-sieve, hopper-sieve, and rake dredge are stated to have been invented by Professor Verrill, and to him also belongs the credit of perfecting the rope tangle for collecting sea-stars and other spiny inhabitants of the sea-bottom. This tangle, or mop, is now used extensively by oyster-growers to catch and destroy the starfish preying on their growing crops of oysters, and without it oyster-culture on a commercial scale would be impossible in extensive areas on our seacoast.

From 1883 to 1886 he had the able assistance of Dr. James E. Benedict, who sorted and provisionally classified the great bulk of the collections, thus making relatively easy the recognition of the many species which were new to science.

After the death of Professor Baird in 1887 these dredging operations were discontinued and Verrill began a summary of the results. Taking up the collections group by group he planned a series of monographs to cover each of the groups of marine invertebrates of New England and adjacent waters. The nemerteans and the planarians were thus completed, while the annelids and other groups remained as unfinished manuscripts at the time of his death.

Having described the more remarkable species of invertebrates found on the New England coast, Verrill turned to other fields which might prove more productive, for his was the spirit of the pioneer, ever seeking new forms of animal life for study. He first selected the Bermuda islands, whence he had received so many of the corals which he had studied. In 1898 and 1901, and again in 1916, he spent some months at these islands. In 1898 he was accompanied by three of his students, and in 1901 by his son as artist and photographer. A large number of marine invertebrates were collected and studied, sufficient material being secured to require several years for its investigation.

The breadth of his research on the Bermuda islands is indicated by the title page of the first of the two fully illustrated volumes which he later published, the title being "The Bermuda Islands; an account of the scenery, climate, productions, physiography, natural history and geology, with sketches of their discovery and early history, and the changes in their fauna and flora due to man." This volume covers 548 pages, with 38 plates and 250 text-figures. The second volume, on the geology and marine zoology of the islands, is of about the same extent. The separate papers which comprise these volumes were first published in the Connecticut Academy of Arts and Sciences and subsequently issued as a private enterprise of the author.

The sponges, the corals and other coelenterates, the land snails and slugs, the insects, myriopods and arachnids, the crustacea and pycnogonida, the echinoderms, the tunicates and molluscoidea were each taken up for study, and the distribution and natural history of each species were described.

For several years prior to 1890 his work on the marine invertebrates was interrupted while he worked on Webster's International Dictionary. To Verrill belongs the credit for the excellence of all the zoological definitions and their accompanying illustrations in this monumental publication. He also co-operated in the supplement to this dictionary, published in 1900.

Verrill's publications extend over a period of about sixty-four years and include more than 300 titles on zoological and geological subjects. That he was able to accomplish so much and in such a wide range of subjects is due not only to his inherited mental ability but also, and in large measure, to his most unusual physique. A man to whom illness or even fatigue was almost unknown from early manhood to his eighty-seventh year, he was capable of a prodigious amount of labor, both mental and physical.

And yet he is said to have been a rather frail child, his health during boyhood being so delicate that his parents feared he might have tuberculosis. He always credited the great vigor of his mature years to the fact that it was so necessary in his boyhood to guard his health most carefully and also to his absolute avoidance of tobacco and all forms of alcohol.

For more than thirty years during the prime of life he was able to keep at the most arduous tasks for many hours a day; in fact it was known to those most closely associated with him that he sometimes worked right through from one day to the next, with at most a brief nap in the early evening. Habits of labor that would speedily have wrecked the life of an ordinary man appeared to contribute to his well-being. But he never used artificial stimulants.

Sleep to him during periods of his most active labors seemed to be in large measure a casual indulgence, taken, quite frequently, while fully dressed and reclining in his desk chair or on a couch conveniently placed in his study. An after-dinner nap from perhaps eight o'clock until nine, then, absorbed in study, writing or drawing, and quite oblivious of time, he would sometimes work until near daybreak; then a little sleep on the couch before breakfast and off to his laboratory. The noon-day meal was often forgotten, while his reluctance to

leave his work frequently resulted in his being from two to four hours late to dinner. Not rarely also the lecture hour passed by unnoticed.

A man of large stature, his massive head covered with abundant locks of wavy hair, and with piercing blue eyes, he made a striking figure in any company. Genial and kindly when he could be persuaded to indulge in social affairs, his self-contained disposition and retiring nature allowed him to make but few intimate friendships.

So marvelous was his memory concerning scientific matters (and yet so poor regarding his personal appointments) that he was able to recall the name and distinguishing characteristics of nearly every one of upwards of a thousand species of animals which he had described as new to science. He was a skillful artist, and had such powers of visualization that with a stubby bit of pencil he could make a satisfactory drawing of almost any species he had ever seen. Finally, his ability to quote references to the literature of the groups on which he worked was truly astonishing. Such an encyclopedic mind not only guarded him against the duplication of generic and specific names already applied, but enabled him to disentangle confusions in synonymy which were baffling to others.

For nearly twenty years after reaching the retiring age limit, in 1907, and when most of his former contemporaries were either dead or living in retired leisure, Verrill continued his studies with unabated energy, publishing in this period a series of papers which constitute in many respects his most important contributions to science. These reflect his maturity of judgment and his accumulated knowledge from so many years of research.

These works summarize his knowledge of the corals and allied animals, the starfishes and allies, and the crustacea, covering more than a thousand pages and illustrated by some two hundred plates. Some time before his death he had placed in the hands of the publishers his most extensive monograph, on the Alcyonaria, consisting of upwards of a thousand pages of manuscript and 150 plates. There is also awaiting publication

a report on the crustacea of Southern New England with over a hundred plates.

Verrill's work was continued almost uninterruptedly until the last few weeks of his life. Even at the age of eighty-five, still sturdy and vigorous, he embarked on a new voyage of discovery on Kauai Island, in the Hawaiian group, with all the enthusiasm that he had shown when Agassiz sent him to Labrador and Anticosti in his student days. Two years were spent at that island, and nearly a thousand lots of marine invertebrates were collected, including numbers of the new species which he was seeking. His remarkable vitality, however, was at last exhausted and after bringing the collection back to New Haven he was unable to continue its study. In the autumn of 1926 he left for California to spend the winter with his son, but he died a few weeks after his arrival. His body lies in Evergreen Cemetery, New Haven.

The honorary degree of M. A. was conferred upon him by Yale and he was honored by being twice appointed as lecturer at the Lowell Institute in Boston. He was a member of the National Academy of Sciences, for some years president of the Connecticut Academy of Arts and Sciences, a corresponding member of the Société Zoologique de France, a fellow of the American Association for the Advancement of Science and a member of the Boston Society of Natural History, American Academy of Arts and Sciences, Wisconsin Academy of Science, Essex Institute, New York Academy of Sciences, Philadelphia Academy of Natural Sciences, California Academy of Science, American Society of Naturalists, American Society of Zoologists and of other learned societies. From 1869 to 1920 he was associate editor of the *American Journal of Science* and he served as professor of comparative anatomy and entomology at the University of Wisconsin from 1868-70 and as a curator of the Boston Society of Natural History for some years, in addition to his professorship at Yale.

In 1865 Professor Verrill married Flora Louisa Smith, a sister of the late Professor Sidney I. Smith, of Yale. Mrs. Verrill died in 1915. Four of their six children survive, the

two sons being Major George E. Verrill and Alpheus Hyatt Verrill.

Several brief accounts of the life and work of Professor Verrill were published shortly after his death. The following contain brief summaries of his principal contributions to science.

Addison Emery Verrill: Pioneer Zoologist, by Wesley R. Coe. Science, vol. 66, 1927, pp. 28-29.

Addison Emery Verrill and his contributions to zoology, by Wesley R. Coe. Amer. Jour. Sci., vol. 13, 1927, pp. 377-387.

Addison Emery Verrill: the life and work of Yale's first Professor of Zoology, by Wesley R. Coe. Yale Alumni Weekly, vol. 36, 1927, pp. 1053-1054.

His genealogy may be found in "Genealogical and Family Histories of Connecticut," vol. 1, pp. 233-238, 1911.

PART 2. CONTRIBUTIONS TO SCIENCE

In an attempt to gain some conception of the zoological influence of the life work of Professor Verrill, there is brought to mind the enormous progress which was made in the science of zoology during his lifetime. Beginning his scientific studies at the time of the arrival of Louis Agassiz in this country bringing the concepts of comparative morphology which were commencing to supplant the earlier systematic work in Europe, Verrill was able to follow the entire course of zoological progress to its culmination in the experimental methods of the first quarter of the present century.

Verrill did not directly participate in these more modern phases of biological research and had little patience with those who were acquainted with animals only under laboratory conditions. He sought always to emphasize the fact that much of the more recent work has been possible only because of the foundations laid by a small group of able men who, since the middle of the last century, have explored the vast fields containing previously undiscovered forms of life and have thereby made known the morphology, natural history and relationships of the organisms available for more specialized and experimental investigation.

Among these pioneer zoologists the name of Verrill stands out prominently because of the amount and accuracy of his con-

tributions to our knowledge of marine invertebrates. A very large number of species, including representatives of nearly all groups, were discovered and described by him, and their relationships to previously known forms were diagnosed with almost unerring accuracy and with a facility that amounted almost to genius.

The exact number of such hitherto undescribed forms which he discovered was not known even to Verrill himself, but he often stated that it was well above a thousand. In later life he expected to summarize his earlier investigations and to treat each group in monographic form but he found time to complete only a few of the groups. His constant urge was to move on into new fields when the outstanding products of the old were exhausted, postponing for a later time the less promising objects. The type specimens of many of Verrill's species are among the zoological collections of the Peabody Museum of Natural History at Yale University. Most of the others may be found at the United States National Museum. A few appear to have been lost to science, for Verrill was sometimes so busy describing his new species that he neglected to label the type specimens, and in some cases they cannot now be found.

I. NATURAL HISTORY, GEOLOGICAL AND BOTANICAL STUDIES

When young Verrill's family moved to Norway, Maine, there was a new environment for the fourteen-year-old naturalist to explore, with somewhat different geological formations than were to be found in the vicinity of his birthplace and here, and in neighboring towns, he discovered deposits of tin-ore, zircon, chrysoberyl crystals of large size, and amazon stone which had not been previously reported. These geological studies were the forerunner of later geological investigations of a professional nature, and it was only through the magnetic influence of Louis Agassiz that he eventually gave to zoology the major portion of his life's work.

Yet he never abandoned his interest in geology and after his appointment at Yale University as professor of zoology he taught physical and historical geology for twenty-four years to large classes of students in the Sheffield Scientific School. Dur-

ing the same years (1870-1894) Professor James D. Dana gave similar instruction to students registered in Yale College.

During several summers he was employed as a geological expert by various coal and iron mining companies and is said to have had a remarkable success in the location of productive properties.

Wherever Verrill went—to Labrador, Anticosti Island, Bermuda, Yellowstone, Hawaii—the geological formations claimed his first attention. The geology of the Bermuda Islands was very thoroughly investigated during his several visits to the islands. One paper on the geology of the islands (No. 82) was published in 1900, while their physiography was fully discussed in his volume "The Bermuda Islands" (No. 89). These were followed a few years later by detailed descriptions of all the geological formations on the various parts of the islands and a comprehensive account of the paleontology (No. 94). The part which each of the various groups of organisms plays in the formation of geological deposits was fully discussed and the organisms themselves illustrated. In the paleontological studies the relationship of the fossil fauna and flora to those of modern times was given particular attention.

Although he made no pretense of his botanical knowledge, he learned to recognize the species of almost every flowering plant which he encountered. He was quick to realize that his boyhood collection of several hundred species contained some that were not at that time recorded for the United States. The plants are enumerated in his report on the natural history of Anticosti (No. 3), and in his volume on the Bermuda Islands (No. 89) 156 species of native flowering plants and most of the introduced varieties are each recorded as to habitat, general interest and economic importance.

Even after reaching the age of 85 years he learned in a few months the names and characteristics of practically all the species of plants growing in the Hawaiian island of Kauai.

2. MAMMALS, BIRDS, FISHES AND OTHER VERTEBRATES

In his boyhood natural history studies, the larger animals of his vicinity naturally attracted young Verrill's first attention,

and his first published papers deal with these groups. These are catalogues of local species and new locality records, but they show remarkable keenness in the discrimination of species and in the observation of the animals' habits of life. One of his early papers on the Geographical Distribution of North American Birds (No. 120) first called attention to the fact that the latitudinal distribution of birds is closely correlated with the mean temperature of their breeding season.

Later papers (No. 79, 131, 132) emphasize the importance of the color adaptations of birds, mammals and fishes for nocturnal protection, a phase of natural selection that had been generally overlooked previously. These were followed by other papers (No. 80, 130) containing important observations on the diurnal and nocturnal changes in colors as protective adaptation in fishes and in the squid. These papers also present observations on the sleeping habits of these animals.

His last papers on birds discuss the mysterious Cahow of the Bermudas (No. 135, 136).

3. CORALS AND OTHER COELENTERATES

Although Verrill gave some attention to practically all groups of invertebrates, his first publication in this field, as well as his last, was devoted to the Coelenterates. His revision of the Polypi of the eastern coast of the United States (No. 140), published the year after his graduation from Harvard, exhibited his keenness in distinguishing the natural relationships of animal groups and this became the basis of future classification.

During the sixty-three years which intervened between his first paper on the corals and the last one which he completed on the Coelenterates some sixty papers and monographs were published by him (No. 138-198a).

Some of his earlier papers on this group discuss also the Echinoderms, for Verrill followed the lead of his illustrious master, Louis Agassiz, in combining the animals which comprise the two modern phyla into a single division, the Radiates. After a few years, however, Verrill himself was one of the first to recognize that the two groups are not closely related

phylogenetically and to place them in widely separated positions in the scheme of classification.

The naked polyps (*Actinaria*), the corals, the sea-fans and sea-plumes (*Alcyonaria*), and other groups in this phylum from both the east and west coasts of America and the West Indies were studied by him, and a large number of genera and species described as new to science. His last and most extensive monograph, on the *Alcyonaria* of the Blake expedition (No. 298), covering upwards of a thousand pages of manuscript and 150 plates, was in the hands of the publishers at the time of his death.

In his expeditions to Bermuda Verrill devoted particular attention to reef-building corals and not only described and figured the numerous species obtained, but made extended investigations concerning their manner of life and their associations.

Among the outstanding morphological features discovered by Verrill may be mentioned the dimorphic zooids in the *Alcyonaria*, the bilateral development of the zooids in the *Zoanthidae*, and the demonstration that the *Tabulata*, as previously understood, was a heterogeneous group, containing an assemblage of forms without close natural affinities (No. 150, 159, 174).

To Verrill we are thus indebted for much of the information which is now available regarding the taxonomy and natural history of this group of animals, and in many respects this constitutes his most successful work. Its importance is due not alone to the numerous new species which he made known to science, nor to the sound system of classification which he devised, but also to the careful observations which he has recorded regarding the mode of life of the many species which he studied.

Perhaps the following papers, selected from his long list of publications on this group should receive particular mention. No. 140, Revision of the species found on the east coast of the United States; No. 142, 143, 148, 158, 163, 173, Synopsis of the polyps and corals of the North Pacific exploring expedition; No. 152, 153, 154, 160, 165, 167, 169, 170, 171, papers

on the Radiata sent to the Museum of Yale College from various parts of the world, and particularly from Brazil and the west coast of America, during successive years; No. 193, Additions to the Anthozoa and Hydrozoa of the Bermudas; No. 194, Variations and nomenclature of Bermudian, West Indian and Brazilian reef corals; No. 195, Comparisons of coral faunae; No. 195a, Corals of the genus *Acropora*; No. 197, 198, Actinaria and Alcyonaria of the Canadian Arctic Expedition. These, together with his extensive monographs on the Alcyonaria of the Blake expedition and on the Gorgonians of the Brazilian coast (No. 196), place on a secure basis the taxonomy and systematic relationships of the groups concerned. A posthumous paper (No. 198a), describing the new species of Anthozoa which he collected on the reefs at the islands of Kauai and Oahu during the last two years of his life, brings his work to a conclusion.

4. SEA-STARS AND OTHER ECHINODERMS

Next to that on the Coelenterates, Verrill's most extensive work was on the Echinoderms, and more particularly the sea-stars. Many important articles were published by him on this group, the most extensive of these being the two volumes on the sea-stars of the Harriman Alaska Expedition (No. 220), with over a hundred plates. The classification was revised and numerous species described as new to science. Other extensive papers treat of the sea-stars of the east coast of North America (No. 44, 52, 146, 211, 212, 213), the West Indies (No. 223, 224), and both coasts of South America (No. 153, 199, 203, 204), as well as of the west coast of North America (No. 205, 206, 207, 217).

Perhaps in no other group of animals are the taxonomic characters more difficult to determine, so that not only is there a lack of agreement among students of the group concerning the natural affinities of the numerous species, but also there is such great variability that it is frequently impossible to define satisfactorily the characteristics which are thought to distinguish a species. This is equivalent to saying that there is either hybridization between some of the so-called species or

that the phylogeny of the starfishes is even at present but little understood (No. 218). Verrill's work will greatly aid in the final solution of these problems.

In the other groups of Echinoderms the situation is less difficult and the new species added by Verrill are in harmony with those previously described by others. The ophiurans of southern New England (No. 34, 211) and those of the West Indies (No. 85, 214, 215, 216) were fully treated, with many interesting accounts of the habits of the various species, in addition to their geographical distribution.

5. SEGMENTED WORMS. LEECHES AND OTHER ANNELIDS

Verrill was one of the first zoologists to study the North American fresh-water leeches (No. 227, 229, 230, 231, 232), describing a number of species new to science.

He later turned his attention to the marine annelids, of which many new species were secured during the dredging expeditions of the U. S. Fish Commission from 1872 to 1887 (No. 41, 52, 71, 76, 229, 234, 235). His contemplated monograph of all the species found on and off the east coasts of the United States had not been completed at the time of his death, although some of the plates had been printed many years previously.

His study of the annelids of the Bermudas was likewise left uncompleted, although he described a large number of new species from the extensive collections which he made during his three trips to the islands (No. 83, 85).

6. CRUSTACEANS AND PYCNOGONIDA

Most of the crustaceans of the U. S. Fish Commission collections were studied by Professor Sidney I. Smith of Yale University and his associates, but after the incapacity of Professor Smith through blindness Professor Verrill added this group to the many others which he investigated.

Three monographs of the crustaceans of the Bermudas were published by him, on the Macrura (No. 244a), the Brachyura and Anomura (No. 244), the Schizopoda, Cumacea, Stomatopoda and Phyllocarida (No. 245). All of these are fully illus-

trated and are remarkable not only for their clear exposition of taxonomic principles but also because they were all published after Verrill had reached his seventieth year and two of them after he had passed his eighty-third birthday. In all of them the distribution, variations and habits of the numerous species, including many new ones, are given particular attention.

Shortly before his death he completed a monograph on the higher crustacea of southern New England, a manuscript of some 700 pages and about 100 plates. Another monograph, on the Decapod Crustacea of Dominica Island, was nearly ready for the printer.

7. MOLLUSKS

Every subdivision of the Mollusca was studied by Verrill, from the tiny marine snails smaller than the head of a pin to the giant squids which reach a length of more than forty feet and are the largest invertebrate animals that have ever existed.

Of this latter group, the Cephalopods, Verrill discovered a number of curious forms in connection with the surveys of the U. S. Fish Commission, and made a very thorough study of their structure (No. 250, 259, 261, 266, 274, 280). He was also fortunate in securing portions of the bodies of some of the largest of the giant squids. These were fully described (No. 252, 253, 254, 255, 256, 257, 258, 260, 265, 267, 270, 271, 273) and illustrated by numerous plates. With the assistance of J. H. Emerton a life-sized model of an animal about 42 feet in length was constructed for the Yale Museum. Copies of this model may be found in the larger museums of the United States and of Europe. A similar model of a giant octopus was also constructed.

In his work on naked mollusks (No. 85, 247, 287), bivalve mollusks (No. 53, 262, 276, 277, 278, 284, 285), and univalve mollusks (No. 262, 276, 278) he was ably assisted by Dr. Katharine J. Bush, for many years assistant in zoology in the Peabody Museum at Yale. Large numbers of new species were described in these groups, especially from the coast of New England and from the deeper off-shore areas.

8. FLAT-WORMS. PLATYLHELMINTHS

Of the monographs on the marine invertebrates of New England and adjacent waters which Professor Verrill intended to publish as a summary of his many years of research, only two were fully completed at the time of his death. These were on the marine Planarians and the Nemerteans.

The Planarians are fully described and their morphological details fully illustrated (No. 290).

All the species of Nemerteans known up to that time are likewise fully described externally, with colored figures of their appearance in life (No. 289, 290a).

In this connection may be mentioned his report on the external and internal parasites of man and domestic animals, which had a wide circulation at the time and was of great service in spreading information about the danger of tape worms and other parasites (No. 27, 33).

9. BRYOZOA AND OTHER MOLLUSCOIDEA

The Molluscoidea are a very ancient group and many of them are of such small size that the group has always been considered difficult of investigation. But Verrill unhesitatingly included this group with the others, describing many new species and untangling a confused synonymy (No. 36, 52, 185, 291, 292, 293).

10. TUNICATES

In his earlier papers Verrill placed the ascidians and other Tunicata among the Mollusca, as was the custom at the time, but later recognized the lack of affinities between the two phyla. Numerous new species are described from his collections along the coast of New England and from the off-shore dredgings of the U. S. Fish Commission. Not all of his supposed species are tenable, as has been more recently shown, but Verrill's descriptions were almost always accurate and his accounts of the natural history of the various species were always reliable.

Perhaps the work which has brought Professor Verrill the widest recognition is the Report on the Invertebrate Animals

of Vineyard Sound and Adjacent Waters (No. 36), published in the Report of the Commissioner of Fish and Fisheries, for 1871-2 (452 pp., 38 pls.). This stands as a monumental work in that it was the first extensive ecological study of the marine invertebrates of the inlets, tide pools, mudflats, estuaries and other physical features of the southern New England coast. It includes the natural history of the sponges, hydroids, echinoderms, worms, crustacea, mollusks and other invertebrates found in each of these habitats, in so far as they could be learned at the time. In these studies he was ably assisted by his brother-in-law, Professor Sidney I. Smith, for many years professor of Comparative Anatomy at Yale University. For more than thirty years this Vineyard Sound Report was the standard book of reference for all students of the seashore life of the region, and even today it is still much used. For the Vineyard Sound region the ecological work started by Verrill and Smith has now been carried to a much higher degree of perfection with the coöperation of a score or more of specialists. It is but natural that the results obtained by this modern practice are vastly more detailed than could be hoped for by the pioneer methods of earlier days.

No small part of the excellence of Verrill's publications is due to the accuracy and artistic merit of the drawings used for the illustrations. Most of the drawings of his earlier papers were made with his own hand, and these display a remarkable dexterity with the pen and faithfulness in depicting intricate details.

During his long connection with the U. S. Fish Commission he was assisted by Mr. J. H. Emerton, an artist who has had few equals in the execution of pen and ink outlines of invertebrate animals. Verrill's later publications were nearly all illustrated by drawings and photographs made by his second son, Alpheus Hyatt Verrill, who undoubtedly inherited his father's artistic ability.

Verrill was also fortunate in having associated with him from time to time young men of ability who later became leaders in their fields. To mention the names of some of these, as Edmund B. Wilson, C. Hart Merriam, E. A. Andrews, Edwin

Linton, Richard Rathbun, Sidney I. Smith, is to indicate that Verrill must have exerted a powerful indirect influence on the progress of zoology in America.

For more than thirty years he had the faithful assistance of Dr. Katharine J. Bush, co-author of several of his papers on mollusks and annelids, whose accuracy and ability are reflected in nearly all of Professor Verrill's publications during that period.

It may be of interest to recall that Verrill lived through practically the entire history of zoology in America; from the coming of Louis Agassiz in 1847, to the experimental period of the present century. But while the vogue of the science changed from taxonomy to comparative anatomy, then to adaptations and the evidences of evolution, and later to biometry, regeneration, cell lineage and embryology and, at the beginning of the century, to experimental fields and genetics, Verrill maintained to the end of his life the importance of taxonomy as a necessary preliminary to this more specialized biological work.

The successive states through which modern zoological discipline evolved had no effect whatever on his own chosen field of research. The natural history of animals, their evolution and their adaptations in nature, had a far greater appeal for him than did any of the methods used in laboratory studies. Partly for this reason and partly because of his retiring habits he was little known to the younger generation of zoologists, and the extent and importance of his work has not yet been fully appreciated.

At the time of his retirement in 1907 President Hadley said of him: "In his investigation of certain large fields of zoology, especially those dealing with marine life, he had no equal in America either for knowledge or for originality, and very few in the world as a whole."

With the exception of the protozoa, the taxonomy of every one of the invertebrate phyla shows the effects of Verrill's labors. In some, the general scheme of classification was modified; in others new genera and species were added. In all, he exhibited what seems to have been a natural intuition as to

the significance of morphological characters which amounted almost to genius. It is but natural that in the hundreds of species which he described as supposedly new to science, a few are now known to have been previously described from other parts of the world and a few others are now classed as mere varieties, but the marvel is that he could have accomplished such an enormous task with so few errors of judgment and with still fewer errors of observation.

There seems little doubt that future generations will accord him recognition as one of America's greatest systematic zoologists and one of the most productive of our zoological pioneers.

PART 3. BIBLIOGRAPHY

The following comprise the principal articles published by Professor Verrill on strictly scientific subjects and which contain material new to science. The numerous book reviews which appeared during the many years when he was an associate editor of the *American Journal of Science* (1869-1920) frequently contained critical comments in the nature of revisions in synonymy, but as most of these were later incorporated in other publications, such reviews are not included here. Nor have there been included the numerous popular articles which he contributed to the newspapers from time to time.

A complete list of Verrill's scientific papers prior to 1893 may be found in Fisher's "Bibliographies of the present (1893) officers of Yale University."

Since Professor Verrill's writings cover so wide a range of subjects, the titles in the bibliography have been grouped under thirteen headings, twelve of which indicate the principal classes of animals which he investigated. The first group of papers includes all the publications which treat of biological matters in general as well as those of his early years which are on botanical and geological subjects. This group also contains papers dealing with two or more groups of animals.

In many cases, however, Professor Verrill allowed his subject to range beyond the limits indicated by the title, for he often followed the example of the earlier naturalists and pub-

lished diagnoses of new species in articles dealing chiefly with other matters. Furthermore, several of the papers discuss the zoology of particular geographical regions and these, of course, treat of several different groups of animals. Thus it will inevitably happen that in some cases a paper placed under one heading will have in it matter which pertains to one or more of the other headings, the paper being listed in the group to which it particularly pertains. In a number of instances it has seemed necessary to list a single paper under two or more headings.

In each list the papers have been arranged in chronological order, and they have been numbered so as to afford ready reference in the text. In those cases where a paper is listed under two or more headings, it retains in subsequent lists the number given to it in the list in which it first appears.

I. MISCELLANEOUS PAPERS, INCLUDING THOSE OF A GENERAL CHARACTER, THOSE COVERING SEVERAL GROUPS OF ANIMALS, AS WELL AS THOSE PERTAINING TO BOTANICAL AND GEOLOGICAL TOPICS

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3. PAPERS RELATING WHOLLY OR IN PART TO CORALS AND OTHER
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4. PAPERS RELATING WHOLLY OR IN PART TO SEA-STARs AND OTHER
ECHINODERMS

In his earlier papers Professor Verrill classified both the Coelenterates and Echinoderms in the group Radiata; hence some of the papers in list No. 3 treat also of Echinoderms. See also list No. 1.

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5. PAPERS RELATING TO ANNELIDS, LEECHES, AND OTHER SEGMENTED
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252. Occurrence of gigantic Cuttlefishes on the Coast of Newfoundland. *Amer. Jour. Sci.*, vol. 7, pp. 158-161, 1874.
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259. Notice of recent additions to the marine fauna of the eastern coast of North America, No. 7. *Amer. Jour. Sci.*, vol. 18, pp. 468-470, 1879. New species of Cephalopods.
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8. PAPERS RELATING TO FLATWORMS AND RIBBON WORMS

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9. PAPERS RELATING TO BRYOZOA (MOLLUSCOIDEA)

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10. PAPERS RELATING TO PROCHORDATA

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- See also No. 36.

NATIONAL ACADEMY BIOGRAPHICAL MEMOIRS—VOL. XIV

II. PAPERS RELATING TO SPONGES

No. 35, 36, 46, 52; 95 (New species of silicious sponges.)

12. PAPERS RELATING TO ROUNDWORMS

No. 26, 27, 33, 36, 52.

13. PAPERS RELATING TO INSECTS

No. 9, 11, 15, 18, 19, 20, 21, 27, 33, 79, 89.

In addition to the publications which appeared during his lifetime, Professor Verrill left completed manuscripts of several works which, for one reason or another, had not been printed at the time when this biography was issued (1930). It seems desirable to list them here since it is presumable that they will later be published, probably with editorial revision and emendations. Such information as is now available concerning them is appended.

297. Report on the Higher Crustacea of Connecticut and Adjacent Waters. About 700 pp., 160 text-cuts and 99 plates.

"This includes descriptions and figures of all the genera and species, the morphology, classification, habits, anatomy, evolution, natural selection, development, lobster hatcheries, and the fisheries, etc."

(This was submitted to the Connecticut Geological and Natural History Survey in 1919, but will require considerable revision in order to conform with the other publications of the Survey.)

298. Deep-Sea Alcyonaria of the Blake Expeditions; 2 vols., quarto, with about 150 plates.

(This is Professor Verrill's most extensive monograph and the one which he considered in many respects the most important work of his life. It is now found to require such extensive revision because of the loss or misplacement of some of the type specimens that the text is to be entirely rewritten and new plates substituted. The monograph will then appear as a Memoir of the Museum of Comparative Zoology, Harvard College.)

299. True Nature Stories for Boy Scouts and other Boys.

"A popular work, relating mostly to the habits of mammals, birds, and other vertebrates, about 300 pages with many text-cuts."

300. Yellowstone Park in winter and spring, with numerous illustrations from new photographs.

301. The Decapod Crustacea of Dominica Island; about 200 pages, 30 plates.

302. Genealogy of the Verrill families descended from Samuel Verrill, 1st, of Ipswich and Gloucester, Mass., and of some related families.

NATIONAL ACADEMY OF SCIENCES

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1870-1927

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BERTRAM BORDEN BOLTWOOD, 1870-1927

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PROFESSOR BERTRAM BORDEN BOLTWOOD was an important figure in the development of radioactivity as a science. His contributions were significant in that so many of them brought forth facts of fundamental importance so that his name is connected not only with radioactivity, but also with isotopy, both of which branches of science gave us new conceptions of physics and chemistry, and with the geologist's problem of determining the ages of formations. There is not a long array of publications by him but each one is the outcome of much experimentation, being comprehensive in statements of procedure and in the results which are very candidly discussed. His love of the laboratory arts and his immense knowledge of them made him the person much sought by those about him in need of assistance in experimental work. On account of his wide acquaintance with sciences in particular and culture in general he was welcomed in groups in which he moved. As a teacher he was connected with Yale for twenty-nine years, about one-half of this time with the physics department and the other half with the chemistry department. His untimely death brought forth expressions of sorrow and of appreciation of his scientific worth from scientists in all parts of the world.

ANCESTRY

His paternal ancestors were of English stock. The American lineage is headed by Robert Boltwood, who immigrated sometime before 1648 and became a freeman in 1658. For a while he lived in Wethersfield (now Glastonbury), Conn., until certain religious difficulties arose, when under the leadership of John Russell he left Connecticut and settled in Hadley, Mass., where he was a miller. His wife Mary is of unknown parentage. Their oldest child and only son was Samuel, who like his father was a miller and was also a sergeant. He fought at Peskeomp-

skut during Philip's War, and also at Deerfield, where he and one of his sons, Robert, were slain in the combat on the Meadows, Feb. 29, 1704. Both were buried with the other victims in a common grave at Deerfield. His wife was Sarah (Lewis). They had five sons and five daughters. Their youngest son Solomon (1694-1762) removed to Amherst about 1737 and married Mary Norton Pantry, with whom he had three sons and three daughters. The oldest son of Solomon, William (1725-1779), settled in Amherst, was a lieutenant and served on the frontier in the French and Indian wars. His wife was Mary Sheldon and they had five daughters and two sons. The younger son, William (1766-1835), was a farmer and a blacksmith at Amherst. William was married three times, his first wife being Eunice Noble. Their second child was Lucius (1792-1872).

William, being a small farmer, was not able to aid greatly his son Lucius in his ambition to secure an education. Lucius, however, worked his way and ultimately graduated from Williams College in 1814. He settled in Amherst, practiced law, and became one of her most influential citizens. He was active in the founding of Amherst College and "his heart was filled with joy and pride when Amherst College became a reality in 1821." He was secretary of Amherst College from 1828 to 1864. He was one of the founders of the Liberty Party and was its first candidate for governor of Massachusetts in 1841. In 1824 he married Fanny Haskins (1807-1888), daughter of Rev. Mase Shepard (Little Compton, R. I.) and Deborah, daughter of John Haskins (Boston, Mass.). Deborah Haskins was a sister of Ruth Haskins Emerson, mother of Ralph Waldo Emerson. In 1835 Lucius Boltwood built a stately mansion (on which spot is located at present the Converse Memorial Library of Amherst College). Hither came the intellectual men and women of New England during the middle of the nineteenth century, to enjoy the hospitality of Lucius and Fanny Haskins Boltwood. A brother of Fanny Haskins (Shepard) Boltwood was the distinguished mineralogist and authority on meteorites, Charles Upham Shepard (1804-1886), a graduate of Yale, connected

with Yale from 1825 to 1847—in the latter years as lecturer in Natural History—and later a professor at Amherst.

The eighth of nine children of Lucius and Fanny Haskins Boltwood was Thomas Kast Boltwood (Feb. 15, 1844, to Dec. 25, 1872), father of Bertram Borden Boltwood, the subject of this memoir. Thomas's middle name, Kast, is the name of the Kast family (Boston, Mass.) of which Dr. Thomas Kast married Hannah Haskins, who was a sister of Deborah and Ruth mentioned above. Thomas Kast Boltwood was a graduate of Yale College, 1864; studied law at the Harvard Law School, at Utica, N. Y., and at Albany Law School. From the latter institution he received the LL.B. degree in 1866. He practiced law in Toledo, Ohio, as a member of the firm Potter and Boltwood. His health broke down in 1869 and later he gave up the profession. He died in Hartford, Conn., in 1872.

The Boltwood family in its various ramifications was quite influential in the community life of Amherst for several generations. Various memorials are to be found in the Grace Episcopal Church at Amherst which connect the names of Boltwood, Shepard, Haskins and Kast. This church was the place of worship of the Boltwood family but Bertram Borden never became a member of it, although in his childhood he attended it quite regularly with his grandparents. The old cemetery (within the town of Amherst) is the resting place of most of the Boltwoods, including Bertram Borden.

His maternal ancestors were of Dutch stock. His mother was Margaret Mathilda (Sept. 25, 1842, to Oct. 12, 1909), daughter of Jeremiah Wall and Margaret (Ostrander) Van Hoesen, and their ancestors were among the early settlers in Rensselaer County, N. Y. The first Van Hoesen came to America in 1644. Bertram's mother was born in Castleton, a village of Schodack, Rensselaer County. She married Thomas Kast Boltwood in 1868. Their first son, Charles, died in infancy and Bertram Borden was the second child. She was considered a very beautiful woman throughout her life. She died in Manchester, England, in 1909, where she accompanied Bertram when he went there to carry on researches with Sir Ernest

Rutherford in the Physical Laboratory of the Victoria University of Manchester, of which laboratory Sir Ernest was the director. She was buried at Manchester.

BOYHOOD PERIOD: 1870-1888

Bertram Borden Boltwood was born July 27, 1870, in his paternal grandfather's home in Amherst. His middle name Borden was given to him for a classmate and friend of his father. The place of his birth became a second home to him and all his summer vacations as a boy in school he spent at Amherst with his grandparents. His mother made her home in Castleton, her native village. She did everything in her power to give Bertram the best education possible. In his early childhood he had a governess and also attended a private school until 1879 when he entered the Albany Academy at Albany, N. Y., to prepare for Yale, and graduated from the Academy in 1889.

There was an unusually strong bond between Bertram and his mother. She was not only a mother to him but a chum and a companion as well. His letters to her from school, college and from his European trips breathe tenderness and love. To her he looked for approval or disapproval of his conduct or of his plans for the future not only through the days of childhood but also later when he was already becoming recognized for his scientific work; and during this later period she gave up her home at Castleton to live with him at New Haven.

As a boy he showed all the traits of a busy youngster, being attracted by sports in which boys are generally interested, loving his fishing rod and the bow-gun and later his camera, and in fact liking very much anything requiring the use of his hands, a trait perhaps common to many boys but in his case seeming to suggest an inherent quality which in later years was so evident to his colleagues and which is so necessary to successful work in experimentation, namely, the mastery of the laboratory arts, which fascinated him and to which he also contributed. Reports have it that he was a bit mischievous and liked to carry a joke to a practical conclusion, but that he was absolutely truthful and honest. A letter written to his grandmother Van Hoesen



BIRTHPLACE OF B. B. BOLTWOOD; HOME OF HIS GRANDFATHER, LUCIUS BOLTWOOD.
AMHERST. IT IS THE SITE OF CONVERSE MEMORIAL LIBRARY, AMHERST



B. B. BOLTWOOD AS A BOY WHILE AT ALBANY ACADEMY

from Amherst in 1882 shows his interests as an active boy but also gives a glance at his character and at his interest in and some knowledge of minerals, which later received much attention from him. He begins by saying that he went to church and he must tell her because he thought it would please her. He describes his activities and also writes: "I have been looking in the ashes of the Walker building* twice, the first time I found nothing, but as good luck would have it I found a good piece of Iceland spar, . . . and another mineral of which I do not know the name." He ends: "I am sorry to trouble you but will you please send my bow-gun and fishing rod if possible. I remain your aff. Grandson B. B. Boltwood."

His mother frequently accompanied him to Amherst but his grandmother Boltwood also took much interest in him and had considerable influence on him. She had the opportunity to see him at play and to observe his pranks and his use of vacation time. It would seem that at times she, who had relatives intellectually prominent, thought that young "Bertie" gave books a second place, while camera, minerals, etc., received considerable attention. Doubtless she had her own interpretations of these observations, and while she realized Bertram's youthful years yet she considered it necessary to draw his attention to fundamentals which she felt he must realize as he was approaching the threshold of manhood. The following portion of a letter written to him while he had still two years at the Albany Academy will be of interest in this connection.

"Amherst, Nov. 8, 1887.

"My dear Bertie,

"I was glad to get a letter from your mother and to learn that you were going on in your studies. I was glad you had been so successful with the photographs of cousins. It is rather hard for one who has such a taste as you have to settle down to books, but if you live you will find that *now* at your age the *foundation* of your future character and standing depends. I have been

*Amherst College Museum, burned 1882.

reading of late the life of cousin Waldo [Emerson] by his friend Cabot. I can see much in Waldo that would fit your dear Uncle Charles, my brother, and I hope some of their noble traits may come down to your generation. . . .”

There is no doubt that Charles Upham Shepard, the noted mineralogist, an authority on meteorites and a chemist, was frequently brought up to Bertram as a splendid example of a scientist, and that Bertram's knowledge of minerals, at the early age of twelve, was not altogether fortuitous. Charles U. Shepard lived his last years in Amherst and his noted collection of minerals came into possession of Amherst College but was destroyed by fire in 1882; it is very likely that young Bertram was impressed by his uncle and his great collection of minerals.

UNIVERSITY PERIOD: 1889-1897

In 1889 he entered the Sheffield Scientific School at Yale, registering for the chemistry course. In his freshman year he received a prize for first rank in physics. Physics as well as chemistry was a favorite with him. During the summer of 1890 he was rewarded with a trip to Europe in company with two friends of his family. He kept a very interesting diary of the whole trip and continually kept his mother informed of the sights, impressions and his doings. Both his diary and his letters to his mother breathe forth unmistakable sincerity of love and affection for his mother. His diary shows a study of the characters of his fellow passengers on the boat and of the people of the various countries. It shows a marvelous comprehension of the people, their customs as well as the scenery of the countries visited. He was independent in his decisions where to go and what to see. When his companions got stalled in Switzerland for mountain climbing in which he showed no special interest, either then or at any time later, he decided instead to spend his time in seeing something of Italy, and having apparently thought of Italy before, he concentrated on Florence and Venice. His summer trip lasted from June 25 to Sept. 25 and included parts of Ireland, Scotland, England, France, Switzerland, Italy and Germany.



PENCIL SKETCH OF B. B. BOLTWOOD WHILE HE WAS IN MUNICH

In his senior year at Yale he took the highest rank in chemistry and read at commencement a dissertation on "Iso-nitroso Cyanacetic Ester." Immediately after graduation, in July, 1892, he left for Germany where he spent two years studying inorganic chemistry at the Ludwig-Maximilian University of Munich, working particularly in special analytical methods and on rare earths under Professor Krüss. Returning to Yale in September, 1894, he was appointed assistant in analytical chemistry (1894-1896) in the Sheffield Scientific School of Yale University. During this period he carried on research work on double salts with Professor H. L. Wells and in organic chemistry with Professor H. L. Wheeler. Late in the winter of 1896 he went to Leipzig, Germany, and spent one semester in the study of physical chemistry in Ostvald's laboratory. On both of the trips to Germany he kept diaries. Reading these one finds expressions of appreciation of German thoroughness in scientific preparation and work; and later also expressions of comparison of Germany with other countries in matters of living and commerce, strongly favoring Germany. This kindly feeling for Germany is expressed even more strongly in his later diaries.

INSTRUCTOR IN THE SHEFFIELD SCIENTIFIC SCHOOL, YALE:

1896-1900

Returning to Yale in the autumn of 1896 he was appointed instructor in analytical chemistry and later in physical chemistry. He received the Ph.D. degree from Yale in 1897, his dissertation involving his research work in "Studies on Chlorides." During this period he devoted his time and energy to teaching and to his own improvement in laboratory technique and to devising new apparatus used in physical chemistry. Physical chemistry at this time was still young and the work of Willard Gibbs was comprehended by rather a limited number of chemists. Facilities for teaching it were not extensive and books, especially in English, were not many. To facilitate his work in teaching his elementary classes Boltwood translated from the German two books, one by Alexander Classen, "Quantitative

Analysis by Electrolysis," and the other by Charles Van Deventer, "Physical Chemistry for Beginners." In the laboratory he was busy improving available apparatus and contributing new types, for example, a simple automatic Sprengel pump and a new form of a water blast. Various pumps needed in his laboratory were improved, and years later when the mercury diffusion pump was just invented he still showed interest in the production of a vacuum by devising a form which in many respects is more serviceable than the original type announced.

He devoted much time to gathering all sorts of information on laboratory arts and technique which served him well in his later work and he seemed to be an inexhaustible storehouse of information in this field to his colleagues and to his students (later) in the Sloane Physics Laboratory and the Kent and the Sterling Chemistry laboratories at Yale. In these laboratories in later years he conducted regular demonstration classes in laboratory arts for research students in physics and in chemistry to better fit these students to solve their difficulties in the technique concerned in their researches. He contributed to the laboratory arts in various ways and always found some means of getting over the difficulties encountered. When a wax of low melting point and fair mechanical properties was required—and none existed at the time—he found a way of producing the "Boltwax," which for a long time was a much desired wax in many laboratories of this country.

While he was engaged in teaching and some research during this period, yet, on the whole, it was still a period of study and preparation and perfection in experimentation. One important piece of work carried on under his supervision in 1899 by one of his students, Mr. Langley, was on the separation of radioactive substances from pitchblende. Boltwood refers to it in his paper on Ionium in giving the history of his work which led to the discovery of ionium. The results showed that Mr. Langley was on the verge of discovering actinium which later was made by Debierne in Paris. These experiments, however, were not immediately continued, mainly because Boltwood severed his

connection with the Sheffield Scientific School and was pre-occupied with other work in his private laboratory.

PERIOD OF GREATEST SCIENTIFIC ACHIEVEMENT: 1900-1910

The most important part of Boltwood's life, viewed from the point of scientific achievement, falls into the period 1900-1910. During the first part of this period he worked in his private laboratory at 139 Orange Street, New Haven, and during the latter part in the Sloane Physics Laboratory of Yale University.

Regarding the first part of this period, Boltwood wrote in the "Biographical Sketches" (1917) of his class of 1892 S. at Yale as follows: "In the year 1900 I severed connection with the Sheffield Scientific School and conducted until 1906 a private laboratory in New Haven. During this period I devoted considerable time to technical chemical work and was for some years associated with Joseph Hyde Pratt,* '93 S. under the title 'Pratt and Boltwood, consulting mining engineers and chemists.' My interest in the scientific side of chemistry remained, however, and I still continued work in these directions." In 1906 he received an offer of assistant professorship of physics in Yale College which he accepted and held until 1910. He says in the "Biographical Sketches": "Although this appointment was in a subject different from the one to which I had chiefly devoted my attention up to that time, it offered increased opportunities for the continuation of my scientific work."

In 1896 Becquerel discovered radioactivity through his investigations of uranium and shortly after that Pierre and Marie Curie with their collaborators made their sensational discoveries in this new branch of science through their investigations of the uranium-bearing mineral pitchblende. This happened during the period when Boltwood was intensely interested in the chemistry of the rare earths and it was in a field of investigation in which he had been studying. It is, therefore, no wonder that he became scientifically interested in these new discoveries.

* J. H. Pratt, later professor of Economic Geology, North Carolina University, Chapel Hill, N. C.

During the first part of the period in the private laboratory with J. H. Pratt, a geologist, he had, no doubt, much to do with analyses of minerals and incidentally started to collect for future scientific analyses all sorts of radioactive minerals.

Radioactivity at that time was not a science as yet, but merely represented a collection of new facts which showed only little connection with each other. These facts needed some correlation and explanation with some basic hypothesis before they could give, as a whole, a semblance of a science. The initial placing of radioactivity on a scientific basis came with the announcement by Rutherford and Soddy in 1903 of their theory of disintegration of radioactive elements. Briefly stated, this theory postulates that an atom of a radioactive element, *e.g.*, uranium or radium, spontaneously disintegrates emitting energy in the form of radiations and that from what is left of the initial atom, an atom of a new element is formed which may in turn disintegrate. This was a bold theory in 1903 but today it is no mere theory but an established fact verified in every case. To this verification Boltwood contributed early and materially. He devoted much time to the investigation of problems dealing with the origin of radioactive elements and with the genetic relationships among these elements.

EQUILIBRIUM RATIO OF RADIUM TO URANIUM

In his papers dealing with the "Radioactivity of uranium minerals," "Ratio of radium to uranium in minerals," and "Origin of radium," he demonstrated that radium must be a disintegration product of uranium. He had analyzed primary uranium-bearing minerals from all parts of the world and showed that the ratio of the activities of radium and uranium was remarkably constant for all old unaltered minerals from various geological formations and with widely varying amounts of uranium content. Since his original work was announced some other results have been published by other investigators in which some variation of this ratio had been obtained; in some cases, as *e.g.* in the carnotite and autunite the mineral was young (geologically speaking) and the equilibrium between radium and

uranium had not been attained or else the ratio based on "bulk" material and small specimens of the same material gave different values of this ratio, indicating some alteration of the material. Boltwood emphasized the necessity of observance of "old unaltered primary minerals" and gave attention to this same problem again in 1920 (with Johnstone) reaching the same conclusion of constant ratio of radium to uranium. This ratio was accurately expressed by him and Rutherford, when the radium standard was established, as 3.40×10^{-7} gm. radium to 1 gm. uranium, and is an important constant in radioactivity.

VERIFICATION OF THE DISINTEGRATION THEORY.

DISCOVERY OF IONIUM

The first deduction from these researches, however, was that radium must be a disintegration product of uranium if the disintegration theory is assumed and because the radium occurred in a fixed and definite proportion to the uranium in old unaltered minerals.

Boltwood was a careful and indefatigable experimenter, repeating his analyses and experiments many times to make certain of his results, but he also had the broad vision to realize that it would be necessary to "grow" radium from its parent substance to give the final proof of his contention.

He reasoned that if uranium, through its first product uranium X (discovered and named by Crookes and now known to be two successive products, UX_1 and UX_2), disintegrates directly into radium, it should be possible to detect in a relatively short time the radium "grown" from pure uranium since the intervening product, UX , is of short life. He tried this experiment by purifying uranium and later at different times examining the solution for radium using the sensitive radium-emanation method by means of which minute quantities of radium emanation, and therefore of radium, could be detected. He found that in 390 days no detectable amount of radium was present. To an average mind such a result would be extremely discouraging or else disproving the contention, otherwise supported, that radium is a disintegration product of uranium.

Boltwood, believing in the disintegration theory, found this negative result an incentive for renewed efforts in experimentation to discover the reason for it. He saw a possible explanation for the disappointing result, namely, the possible existence of an element of a long-life period between uranium X and radium. If this should be true then this long-lived element would retard the production of radium so that growth of radium might not be detectable in the short space of 390 days, the time of his experiment. However, if the hypothesis is correct, this intervening element must be present in an equilibrium amount in the same mineral which yields the uranium and the radium. He promptly started to search for this hypothetical "parent of radium." He gave attention to the actinium element discovered in uranium minerals by Debierne and announced by him in two papers of the *Comptes Rendus*, Paris. Boltwood makes this statement: "In these papers, which were characterized by the lack of precise experimental details and the absence of explicit statements, the chemical properties were described as very similar to those of thorium, from which it had not been found possible to separate it completely." Boltwood, in his work, followed a method in which a precipitate was made with thorium. The substance separated, thought to be actinium, was used to "grow radium." This substance did produce radium and for a while Boltwood thought that actinium, or at any rate what Debierne separated with thorium and called actinium, was the missing parent of radium. Performing many experiments on the separation of actinium he finally found that actinium could be separated with some rare earths and that the thorium could be separated from the actinium and also purified by repeated precipitation (with sodium thiosulphate) so that ultimately he had the actinium with the rare earths and the thorium free of actinium. The "growth of radium" experiments revealed that actinium did not grow radium whereas the thorium precipitate did. The thorium was added to his solution of the carnotite mineral in the experiments when this mineral was used. Evidently, a substance was separated with thorium which proved to be the parent of radium. Further investigation showed this

substance to be unlike any known radioactive element, that the α -particles which it emitted had a definite range of their own and that it produced radium in amounts proportional to the time of growth. He was now certain that he had discovered a new radioactive element, the sought-for parent of radium. He proposed to name it Ionium—"a name derived from the word ion. This name is believed to be appropriate because of its ionizing action which it possesses in common with other elements which emit alpha radiation."

It is almost a certainty that some of the material which Debiegne separated with thorium and which he thought was actinium must have contained more ionium than actinium. Had Debiegne followed up his thorium precipitates he might have discovered the element now known as ionium. On the other hand, if the work of Langley, Boltwood's student in 1899, had been immediately followed up, actinium might have been announced from New Haven instead of from Paris.

The original experiment of growing radium from purified uranium was still necessary for a complete proof of radium's genetic descent from uranium. The solution originally prepared was examined about four years later and showed a growth of ionium but the activity was too small to yield a quantitative proof. However, Soddy's experiments in 1919 along the same line on material about twenty years of age gave conclusive proof on this point and also a fairly accurate determination of the half-value period of ionium. Boltwood's material prepared at that time and sealed to be examined after a long period of time is extant and will be examined in due time.

HALF-VALUE PERIOD OF RADIUM

The knowledge that ionium is the direct parent of radium and that it is a disintegration product of uranium and that all these elements are in the same given uranium-bearing mineral, suggested to Boltwood a method of finding the "average-life" of radium and from this, of course, the half-value period and the characteristic constant. The method consists in separating the radium and the ionium from an old unaltered uranium-

bearing mineral in which the uranium, ionium and radium are in radioactive equilibrium. The rate of disintegration of the ionium found in the mineral is equal to the rate of growth of radium from the same ionium and this rate of growth is equal to the rate of disintegration of the radium found in the mineral because of the equilibrium condition. Consequently, by growing radium from the ionium for a suitable time, the rate of growth can be obtained and knowing the radium in the mineral, the "average life" is deduced. The comparison is made by employing the radium emanation method. The method of obtaining the life of radium in this manner has the advantage over other methods in that no standard of radium is necessary. His was one of the first experimental determinations of this constant. Miss Ellen Gleditsch carried on these determinations some years later under Boltwood's direction at Yale and later at Oslo, growing the radium over longer periods for more accurate comparisons and her value of the half-period is 1690 years, which is undoubtedly our best value of this constant.

CHEMICAL INSEPARABILITY OF IONIUM AND THORIUM. ISOTOPY

The very extensive and careful chemical work done by Boltwood in connection with the discovery of ionium brought out a very important scientific fact which was so startling that many chemists and physicists seized upon it as a clue to other investigations the results of which form the foundation of a new branch of the physical science, namely, Isotopy. Boltwood separated ionium with thorium. In some cases he used large quantities of thorium and then tried to separate these two elements from each other. He did an immense amount of work hoping to find a way of separating ionium from thorium, using every known and conceivable method. He wrote on this point, among other things, under the heading "Chemical Properties of Ionium": "The separation of ionium from thorium presents indeed a difficult problem and I have been unable to discover any indication that even a partial separation can be effected by the use of such characteristic reactions as the precipitation of the thorium by hydrogen peroxide, sodium thiosulphate, meta-

nitro-benzoic acid or fumaric acid. From its position with respect to radium it can be safely assumed that the atomic weight of ionium is probably not far from 230, and the atomic weight of thorium, 232.5, would bring these two elements into close proximity in the periodic table." What he found was the important fact that when ionium and thorium were once mixed, they were then chemically inseparable. Aston, in his book on Isotopes, puts it as follows: "The chemical similarity between these two bodies was therefore of an order entirely different to that exhibited by the rare earth elements, and came as near absolute identity as the most critical mind could require."

In this connection, some of his work on thorium salts and thorium-bearing minerals is also of importance. Hahn had discovered radiothorium and found its half-value period to be two years. Boltwood's study of the activities of thorium oxide prepared from commercial thorium salts and also from various minerals (including the monazite sand from which his commercial salts were produced) showed that the oxides from the commercial salts were only about half as active as the oxides from the minerals. He naturally supposed that the commercial process removed some of the radiothorium which was thought to be directly produced by the thorium disintegration. Data from his experiments showed in that case (*i. e.* if radiothorium is produced directly from thorium) that the half-value period of radiothorium should be nearly six years, and he communicated his results to Hahn. Hahn, attempting to reconcile the two differing values, suggested that an intervening element must exist between thorium and radiothorium and that it was this intervening element which was separated by the commercial process. Hahn searched for it and discovered it and named it mesothorium, deducing for it a half-value period of 5.5 years. Boltwood studied the chemical properties of mesothorium and showed that it can always be separated by exactly the same chemical processes as are used to separate thorium X. This result is in fact identical with the one noted for thorium and ionium.

At that time he was occupied with the experimental proof that ionium is the parent of radium and he did not follow up the work just referred to. However, Marckwald, Keetman, Auer v. Welsbach, Soddy and others did follow the clue suggested in Boltwood's "inseparability" of certain chemical elements, one of which, at least, was always a radioactive element, and they added much information on the similarity if not identity of chemical and physical properties of various radioactive and non-radioactive elements. A. S. Russell and Rossi working in Ruthenford's laboratory and using material produced by Boltwood showed by their spectroscopic experiments that the spectra of ionium and of thorium must be the same, since the admixture of ionium to thorium brought out no spectral lines not found in the spectrum of thorium alone. Within a few years it was demonstrated that there are chemical elements possessing the same chemical and physical properties differing only in the atomic weights and radioactive properties (if radioactive), and that such elements would occupy the same place in the periodic table of chemical elements. Soddy, who contributed much to this subject, coined the word "isotopes" designating such elements.

LEAD AS FINAL INACTIVE DISINTEGRATION PRODUCT OF URANIUM

Boltwood was very early impressed with the fact that all his analyses of uranium-bearing minerals showed the presence of lead. On reviewing various published analyses of minerals containing notable proportions of uranium he was further impressed with the almost invariable report of the occurrence of lead. Communicating on this matter with W. F. Hillebrand of the U. S. Geological Survey, whose analyses of uranium minerals have been many and of recognized authority, Boltwood states that he was particularly impressed by the information, supplied in a private communication, "that so far as his [Hillebrand's] experience goes he does not remember to have found uranium in any mineral without its being accompanied by lead, and he adds 'the association has often caused me thought.'" With his usual scientific zeal Boltwood wanted to make a cer-

tainty of this apparent fact feeling it to be of great consequence. Under "Lead" in his first paper on the "Ultimate Disintegration Products of Radioactive Elements," he writes: "Out of a considerable number of analyses undertaken with the particular object of discovering whether or not lead was present, I have been unable to find a single specimen of a primary mineral containing over 2 per cent of uranium in which the presence of lead could not be demonstrated by the ordinary analytical methods." Farther on he says: "Through a dawning appreciation of the significance of the persistent appearance of this element in uranium minerals, the writer was led to suggest in an earlier paper* that lead might prove to be one of the final inactive disintegration products of uranium. All the data which have been obtained since that time point to the same conclusion." In his later work he reported much to strengthen this conclusion.

What we know now is that the final product, radium G, of the uranium-radium series is an isotope of common lead, with the same chemical properties. The work of Richards and of others on the isotopes of lead makes this definite; and the application of the displacement law to the elements of the uranium-radium series places the final product, radium G, in the same place as lead in the periodic table. The theoretical atomic weight of this product comes out 206.17 when we assume that of uranium to be 238.17 and of each of the eight α -particles emitted in the disintegration to be 4. Richards and his pupils at Harvard, Hönigschmidt in Prague and in Vienna, and Miss Gleditsch (at one time a research student under Boltwood at Yale) in Oslo have determined the atomic weights of the lead in some particular old primary uranium-bearing minerals and found values of 206.046 to 206.2, while the atomic weight of ordinary lead is 207.20. These values are for all the lead isotopes, including actinium D and perhaps traces of thorium D and common lead, but mainly radium G. These results are a sufficient verification of Boltwood's contention since isotopy had not then (1905) been brought to light.

* Phil. Mag. (6) 9: 599-613 (1905).

THE AGE OF MINERALS

Desiring to furnish further proof of his contention, he set down the following conditions to be proven: "In unaltered primary minerals of the same species, and of different species from the same locality, that is, in minerals formed at the same time and therefore of equal ages, a constant proportion must exist between the amount of each disintegration product and the amount of the parent substance with which it is associated. And, in unaltered primary minerals from different localities, the proportion of each disintegration product with respect to the parent substance must be greater in those minerals which are the older and should correspond with the order of the respective geological ages of the localities in which the minerals have been found. It also follows that in secondary minerals, namely, in minerals which have been formed by the subsequent alteration of the original, primary minerals, the relative amounts of the disintegration products must be less than in the primary minerals from the same locality, provided, however, that the disintegration products can not be considered as original chemical constituents of the secondary mineral." His second paper on the "Ultimate Disintegration Products of the Radioactive Elements. Part II. The Disintegration Products of Uranium," deals with this problem and the data given for the ratio of lead to uranium, not only from his own analyses but from many of Hillebrand's and others, convince him that the conditions set above have been met and that they constitute evidence that lead is the final disintegration product of uranium.

The "geological ages" as used so far were really only relative and not quantitative values. To express them in years he proceeded according to the following plan: "Knowing the rate of disintegration of uranium, it would be possible to calculate the time required for the production of the proportions of lead found in the different minerals, or in other words the ages of the minerals." From the rate of disintegration of radium and the relative ratio of radium to uranium, he deduced the weight of uranium in one gram of uranium which would be transformed in one year into lead and divided the lead to uranium

ratio by this quantity. This first calculation was approximate and was stated to be such.

In this work Boltwood laid the foundation for the best method we have today in calculating the "age of the earth." With our present knowledge of the isotopes of lead we find that we are obliged to modify his simple calculations in order to take care of these isotopes, namely, radium G, thorium D, actinium D and common lead. At the time of this work Boltwood did not believe that thorium disintegrated into lead, basing his reasoning on the small amounts of lead generally found in thorium-bearing minerals, not knowing the very much slower rate of disintegration of thorium compared to uranium; and furthermore, the final product of the actinium disintegration was not known. By the application of similar reasoning to the disintegration of thorium and of actinium that Boltwood used in the case of uranium and by determining an extra (important) quantity, namely, the atomic weight of the lead isotopes found in the primary unaltered mineral, it is possible by a somewhat more complicated formula to deduce the age of the mineral.

He also gave attention to the helium found in minerals and by collecting the available experimental evidence he concluded that "the amounts of helium found in radioactive minerals are of about the order, and are not in excess of the quantities, to be expected from the assumption that helium is produced by the disintegration of uranium and its products only." In his calculations he used the equation that uranium (238.5) = lead (206.9) + helium (31.6) *i. e.* "that for every 207 parts of lead there will be formed 32 parts of helium." It must be noted that it was then not yet definitely proven that helium was the result of the emitted α -particles. Discussing the results he points out that "in general with greater density of the mineral a greater proportion of the total helium formed has been retained within it." The evidence is in support of the idea advanced by Rutherford in St. Louis in 1904 that the helium content of uranium minerals may be used to indicate at least the minimum age of these minerals.

GENETIC RELATION OF ACTINIUM AND URANIUM

Studying the activities of the various radioactive constituents of a uranium mineral, Boltwood found early that actinium was as regularly present as radium but that its activity was very much too small to be in a direct genetic relation: uranium-radium. He writes: "Insofar as these experiments throw any light on the question of a genetic relation between actinium and uranium, I think that the constancy of the activity of the different minerals and the fact that quite appreciable amounts of actinium can be separated from all of them make it necessary to assume that the amounts of actinium in a mineral are proportional to the quantity of uranium present. It is therefore extremely probable that actinium is a disintegration product of uranium, although its position in the uranium-radium series is still to be determined." Rutherford, in his Silliman Lectures at Yale in 1905, tried to explain the anomalous relation by suggesting that actinium was a branch product of the main series. Such branchings have since been observed in several places and indicate to us that the complex structure of the atomic nucleus may break down in at least two ways. Under such a supposition the branching may take place at U I or at U II, and generally was assumed from the U II for various other reasons. Boltwood in 1920 in a paper with Johnstone discussed the various possibilities and in view of our incomplete knowledge of the subject left the question open, although inclining to the above mentioned branching, but explicitly stating that it is not impossible that actinium may be a product of an uranium isotope independent of genetic relation to U I and U II. From the recent results of Aston (1929) on the atomic masses of the constituent isotopes of lead from a bröggerite, who found one of atomic mass 207, the principal one being 206 (radium G), it would seem that the actinium came from an isotope (239) independent genetically of U I and U II but mixed with these two. Nevertheless, even if this be true for minerals of much the same age, constancy of activity of actinium to uranium would still be observed as seems to be fairly well established. Much work is left to be done here and the experimental determination of the

atomic weight of actinium, in particular before the exact relation of actinium and uranium is established; but from Boltwood's original work we feel certain that a close relation does exist.

OUTSTANDING CONTRIBUTIONS

These outstanding contributions of Boltwood may be summarized as follows:

(1) Discovering a radioactive element, ionium, the parent substance of radium;

(2) Proving the direct genetic relation of uranium, ionium and radium;

(3) Showing that certain elements have identical chemical properties by the chemical inseparability of these elements—a fact forming the starting point of many observations by chemists and physicists from the results of which isotopy had arisen;

(4) Providing evidence that the "lead" found in unaltered primary minerals must be the final inactive disintegration product of uranium;

(5) Devising a method for the calculation of the age of uranium minerals from their lead and uranium contents;

(6) Showing that actinium is genetically related to uranium but not in the same line as radium.

OTHER CONTRIBUTIONS

The above contributions bear the mark of fundamental importance in the physical sciences. Besides these noteworthy investigations, he was first in this country to devise an accurate and quantitative method of investigation of the radioactivity of spring waters. He pointed out that it was necessary to boil the waters (instead of bubbling air through them) to release all the emanation for quantitative measurements. In the absence of radium standards (1904) he was able to give quantitative significance to his measurements in terms of uranium. His standard for comparison was a solution of an old unaltered mineral for which the radium to uranium ratio was known and of which the uranium content had been determined. A known quantity of

this solution, therefore, contained a definite amount of radium, the activity of whose radium emanation (radon) was compared with that found in the water studied. In this way the emanation of the water per liter could be expressed in terms of the uranium in grams of his standard. He investigated various natural waters near New Haven and in 1904, at the direction of the Secretary of the Interior, made a survey of the waters of Hot Springs Reservation, Arkansas, not only for the radium emanation but for radium itself held in solution in the various waters.

During his sojourn in Manchester he carried on further research on chemical separations of radioactive substances, in particular of ionium and of actinium. With Sir Ernest Rutherford he studied the production of helium by radium, polonium and ionium to verify the conclusion that all the helium found with these radioactive substances is due to their emitted α -particles.

He thought a great deal on the philosophic and speculative questions that arose in his mind with the discoveries made but the scientist within him kept a severe check on his flights of thought to stay within the realm of scientific knowledge and probability. The following quotation from his "Ultimate Disintegration Products" may be of interest in this connection. Having finished with the discussion of the facts about the various substances found in uranium minerals, he writes next to the final summary under the heading of "Origin of Elements" (1905): "If it can be ultimately demonstrated that lead, bismuth, barium, hydrogen and argon, or any one of them, actually result from the disintegration of uranium, an interesting question which naturally arises will be: Have the quantities of these chemical elements already existing been produced wholly in the same manner? Any discussion of this problem at the present time would certainly be premature, but the time may not be very far remote when the question will deserve serious consideration."

In 1909-10 he was granted a leave of absence from Yale and he spent the year with Rutherford in the Physical Laboratory of the University of Manchester where he held the John Harling Fellowship for that year. It was at this time that I first became



BOLTWOOD'S CHARACTERISTIC SMILE: IN HIS LABORATORY IN SLOANE
PHYSICS LABORATORY FALL, 1917

acquainted with him, for I had also come to work in Rutherford's laboratory. His joviality and openheartedness made immediately a strong impression on me and we became and remained intimate friends.

In October of 1909 his mother, who accompanied him to Manchester, died there. Those who knew him and his mother and their mutual devotion to each other felt keenly the severe blow that fate had dealt him. Outwardly, he seemed to bear well under it but some of the letters from his New Haven friends to him, at this time, seem to indicate that in his letters to them he showed almost a broken spirit.

He received an invitation to continue his researches in England, but he finally decided not to accept the invitation. He returned to Yale as professor of radio-chemistry and was assigned, as before, to the department of physics where he was associated with Professor H. A. Bumstead. Together with Professor Bumstead he devoted much time and energy to the building of the Sloane Physics Laboratory, which was completed in 1912. During the college year 1913-14, in the absence of Professor Bumstead, he was acting director of the Sloane Physics Laboratory. His connection with the physics department continued until 1918, when, on the retirement of Professor F. A. Gooch, he was appointed acting professor of chemistry in Yale College, 1918-19, and acting director of Kent Chemical Laboratory, 1919-22. During the war he served as an instructor in the Yale R. O. T. C. in 1917, and in 1918 he was in charge of the chemical unit of the S. A. T. C. He was also engaged in experiments on the detection of submarines at the submarine base at New London.

For the first year after the organization of the university department of chemistry in 1918 he was the chairman of the department. During the next four years he was occupied chiefly with undergraduate teaching and with administrative work, including planning of a new laboratory which was to house the reorganized department of chemistry.

The planning of the Sterling Chemistry Laboratory, first projected by the university in 1920, and its construction and equip-

ment involved the application of much time and labor to the consideration of the innumerable technical details. Acting as the representative of the chemistry department in these matters, a considerable proportion of this work fell to his lot and constituted his principal occupation during the years of construction and equipment. Boltwood had thus much to do with two important laboratories of Yale University, the Sloane Physics and the Sterling Chemistry. The work with the latter was so severe that his health broke down under the strain and in 1924 he was forced to ask for a half-year leave of absence to regain his health. He spent this time principally at Chapel Hill, North Carolina, where his former partner in his private laboratory in New Haven, Joseph H. Pratt, is professor of economic geology at the University of North Carolina. The rest from work, change of scenes, and renewal of former friendship with his friend, had a salutary effect on his health and he returned seemingly in good health and cheerful spirits. He applied himself to research with zeal and carried on work on the chemical separation of thorium and other radioactive elements and his notebooks show a large amount of work done. However, his recovery in health was apparently not complete, for he had two more periods of depression and although he again apparently recovered, there was a recurrence of this to some degree again in the summer of 1927, when he ended his own life in Maine where he went to recuperate. He died during the night of August 14-15, 1927, at Hancock Point, Maine.

Boltwood was liked by his friends for his great personal charm and his wide acquaintance with things outside of his science. He found good in everybody with whom he came in contact and was always ready to spend his time and his energy in giving assistance. He had a great personal influence on students and he associated with them a great deal. He took interest in their sports but he himself participated only very moderately and when he did he gave preference to swimming, sailing and walking. His hobbies were photography and in later years "playing" with radio circuits.

In regard to his own work and his contributions to science he was amazingly modest. For biographical works for which he was asked to supply personal data he gave, regarding his work, only: "Contributor to scientific journals." For all that, his contributions speak for themselves and the scientific world knows their worth and gives him its appreciation of it. He remained unmarried.

Boltwood was a member of the National Academy of Sciences (1911); the American Physical Society; the American Chemical Society; the American Philosophical Society of Philadelphia; the American Academy of Arts and Sciences of Boston and the Connecticut Academy of Arts and Sciences. At Yale he belonged to the Sigma Xi, Aurelian Honor Society, Book and Snake and the Cloister Society.

A bronze tablet was placed, in July, 1929, to his memory in the halls of the Converse Memorial Library at Amherst College. It reads:

IN MEMORY OF BERTRAM BORDEN BOLTWOOD

July 27, 1870—August 15, 1927

Born in the house of his grandfather on the site of this building and later professor in Yale University.

He was

The Discoverer of ionium and its genetic relation to uranium and radium,

The Demonstrator of the chemical inseparability of certain elements which is the basis of isotopy, and

The Developer of the radioactive method for estimating the age of the earth.

SCHOLAR TEACHER SCIENTIST

BELOVED AND HONORED FRIEND OF ALL WHO
KNEW HIM

Sources used for this memoir are as follows: The scientific papers of Bertram Borden Boltwood as given in the appended bibliography; scientific notebooks, diaries and personal papers left in his estate to his heir, Lansing V. Hammond; the various genealogical records and papers pertaining to the Boltwood family in possession of Miss Fanny Haskins Boltwood, Goshen, Massachusetts; private information relating to Van Hoesen family from Bertram B. Boltwood's aunt, Miss Albertine Van Hoesen, New York City; "Ralph Waldo Emerson: His Maternal Ancestors" by David Green Haskins, published by Cupples, Upham and Co., Boston, 1887; records of Yale University, Albany Academy and Amherst College; Boltwood's autobiographical sketch in "Biographical Sketches, Class of 1892 S, Yale"; "Who's Who in America," volume 13; "American Men of Science," 3rd ed.; biographical sketches prepared by me for the *Yale Scientific Magazine*, volume 2, 1927, for the *American Journal of Science*, 5th Series, volume 15, 1928, and for the "Dictionary of American Biography," volume 2, 1929; and besides these, also my personal knowledge of Boltwood beginning with acquaintance in Sir Ernest Rutherford's laboratory at Manchester, England, in 1909, which developed and continued as intimate friendship through the rest of his life.

I desire to extend my thanks to those who assisted me by giving me information and permitting me to use available records in their possession, and in this respect especially I wish to thank Mr. Lansing V. Hammond and his mother Mrs. Fanny Reed Hammond, Miss Fanny Haskins Boltwood and Miss Albertine Van Hoesen.

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JOHN MERLE COULTER

November 20, 1851—December 23, 1928

BY WILLIAM TRELEASE

Heredity and environment both contributed to making John Coulter what he became. He was born at Ningpo, in China, where his parents were engaged in religious missionary work. Widowed when he was only two years old, his mother returned to her earlier home in Hanover, Indiana, where her two sons—for his younger brother, Stanley, also had been born in Ningpo—grew up tutored and precepted by a cultured mother and stimulated by attractive and refining surroundings.

Among American botanists who have achieved distinction he falls on the transition line between those to whom preparation for earning a livelihood lay apart from training in botany as a vocation, and those to whom such training is professional. Liberally rather than specially educated, he had found himself in the field of his life work before obtaining the doctorate in philosophy, which is considered the open sesame to preferment in academic circles today though it was all but unknown among American botanists when his work began. His own scholarship was stamped with the academic approval of Hanover College, which gave him the bachelor's degree in 1870 and the master's degree in 1873, and of the University of Indiana, from which he won the degree of doctor of philosophy in 1882 and received the honorary doctorate of laws in 1920.

Coulter was richly endowed by nature for a successful career as an educator. An impressively large and comely man, he demonstrated early his unusual talent for clear analysis and clean-cut presentation of subject-matter, which, though normally free from polemics and personal criticism, neatly disposed of contradictory opinions and left in the minds of his hearers that sense of finality dear to the student hearer; and his was the rare faculty of holding the reins of the teacher with that combined firmness and gentleness which guides without any sense of compulsion, so that from the earliest to the latest those

of his students who survive him hold him in affectionate as well as reverential memory.

One of the most evasive as well as most significant points in every successful life is that at which a life-passion appears. This turning point in John Coulter's case came in his senior year at Hanover College, when—as Mrs. Coulter has written charmingly—a young man came to the campus to teach the natural sciences, Professor E. Thomson Nelson, who “had a lively interest in hunting down the flowers about the lovely surrounding hills. John was his almost constant companion, and, as he seemed to be equally interested in everything he studied, became for the time being a ‘botany fiend’”: to this localized first interest, it may be remembered, he returned frequently in later years. Those of us who knew him in later life were aware that he limped slightly in walking. Through his college life this impediment, resulting from a very serious wound in one knee, seems to have been far greater, and his field activities are the more noteworthy because at this time he was compelled to use crutches and was neither strong nor with that evidence of sturdy health familiar to those who knew him later.

To Dr. Frank H. Bradley, Nelson's predecessor at Hanover, and at that time his preceptor in geology, is credited the start toward health and fame in Coulter's life. Bradley was appointed by Hayden when the United States Geological Survey was organized in 1872 and he gave Coulter an opportunity to go into the field with him as an assistant in geology—an outing that returned him to the haunts of men permanently reconstituted.

Well schooled, he had begun his teaching career, immediately after graduation, in a girl's school at Logansport, Indiana, associated with his mother, where he taught Latin nominally, with incidental inclusion of “Greek or any study that needed a teacher.” Knowing his educational environment, it is easy to see how easy the transition was from the Latinist to the geologist. The conversion of the geologist into the botanist is as clearly forecast in the same knowledge—given only the opportunity.

Some of those who have heard Coulter lecture on his early western experiences have heard him explain, whimsically, that this opportunity came through his inability to play cards.

The story is that whatever tasks he may have performed as a geologist-assistant on the Survey, he could not refrain from collecting the odd plants—very unlike those of the Mississippi Valley—that he found in this “Far West.” When the daytime activities of the organizing field party were ended, Coulter found his evening diversion in giving to plants picked up here and there through the day that affectionate looking-over that every real botanist knows to be the prime reason for putting them in press. One evening when he was thus trifling with his specimens while his companions were engaged in the more generally interesting pastime of a social game, a voice from over his shoulder inquired what sort of a game of solitaire *that* was. This was early in the season, at Ogden, Utah, and the voice belonged to Hayden, who had dropped in unannounced, and who had the good sense to harness in an unrepresented field the enthusiast that fate had played into his hands, even though he could not cripple the geological side of the Survey by relieving him from the work for which his original appointment was made.

The immediate result of the opportunity this gave Coulter may be read in the published results of his work on the Hayden survey; a “Synopsis of the Flora of Colorado,” prepared in collaboration with Professor T. C. Porter (1874), and a report on the botany of Montana, Idaho, Wyoming and Utah (1875).

For Coulter, himself, the opportunity meant much more than this, and its fruits were far-reaching. Critical study of a flora very different from that of the Eastern States necessarily brought the young botanist into correspondence and personal relations with the eminent botanists of the day, whose friendly aid continued so long as they lived.

Out of the Colorado synopsis grew a formal “Manual of the Botany of the Rocky Mountain Region” (1885), modeled—like Chapman’s “Manual for the Southern States”—largely after Asa Gray’s masterly “Manual for the Northeastern States”; and this Rocky Mountain manual was brought more nearly to

completeness in editorial cooperation with Aven Nelson a quarter century later.

The welcome accorded this Rocky Mountain manual, with its short diagnostic descriptions which included, as Coulter once phrased it when speaking of another book of the kind, "enough but not too much"—seems to have inspired the idea of putting into similar form an account of the plants of the Mississippi Valley and of the plains, in large part omitted from his own work, from its often revised prototype—Gray's manual, and from the already antiquated southern manual of Chapman for which demand had become so slight as seemingly to warrant only inadequate supplements instead of a thorough revision.

The scope of such a work fell too close to the natural limits of both of these books, and Coulter was drawn into consultation with publishers and author concerning an extended and modernized edition of Professor Gray's manual, in the preparation of which Coulter should cooperate and bear the brunt of the labor. Incorporation of the South proved quite too large an undertaking for a handbook of the approved size and cost; but within their own parallels it had been virtually decided that the Rocky Mountain and eastern floras should be brought together by a westward extension of range for the new edition of Gray's manual. Professor Gray's death ended the original plan but a less critical revision on much the same geographic lines was undertaken and carried through by Sereno Watson and Coulter with a methodical promptness which possibly would not have been possible if Dr. Gray had lived. The significance of this statement perhaps will appear to those who have used successively the last of the editions of this manual prepared by its author, the revision within strict limitations of size by Watson and Coulter, and the latest and very different edition, by Robinson and Fernald, which followed in due time. Quite apart from obvious new discoveries in even the well explored East, formal segregations in what had passed for species were claiming recognition, and Eichler's views on classification were affecting the sequencing of plant families.

It was here that Coulter entered upon a second evolutionary stage as a systematist—that of the monographer; for when the revision of the eastern manual was under discussion he had progressed far in a critical analysis of the North American Hypericaceae; and a comparable study of our Umbelliferae, in conjunction with his most productive student in the systematic field, J. N. Rose, was well under way before the new manual had been brought to a conclusion. In the same line of study, and similarity as a guiding senior author, Coulter successively worked our Cornaceae, Amaranthaceae and part of the Cactaceae, recurring from time to time to each group as new discoveries or interpretations prompted; and in conjunction with Rose he was tempted into a further investigation of leaf anatomy as a taxonomic and diagnostic guide in certain Coniferae for which Engelmann had somewhat more than broken the way.

Coulter's talent for seizing and epitomizing succinctly the high points of specific characters, which Asa Gray's writings had made a second nature with him as with many others, brought to him a number of Central American plant groups represented in the collections made or stimulated by Captain John Donnell Smith; and his study (1889) of an extensive Texan collection led him even to prepare a preliminary "flora" of Western Texas (1891-1892), in a way probably giving form to his wish to see the floras of East and West brought workably together.

Monographic or "floristic" work necessarily implies the accumulation of a considerable amount of herbarium material without which, indeed, it lacks permanent verifiable value. During that part of his life in which such work engrossed much of his attention, Coulter did not escape the impulse and need of forming an herbarium, the major part of which, following the several removals of a lifetime, is housed now in the Field Museum in Chicago.

While browsing in the floristic field into which he had strayed rather than been guided, Coulter seems to have experienced the enthusiastic pleasure of every young amateur, in the true sense of the word. New "finds," interesting plants, and lists contributory to a final accounting for the higher plants of

Indiana and the western mountains, comprise the usual topics of his publications; here and there a reaction of the teacher on laboratory methods, crystals or some physiological subject, a few observations on teratological deformations, and one note on dichogamy—the only evident effect of that vitalizing interest in field observation that Darwin's beautiful studies were making known in the seventies and eighties of his century.

Perhaps the most venturesome act of a life prolonged well beyond the three-score-years-and-ten, was Coulter's establishment of a journal for the publication of this sort of casually interesting observations, to which for a time he was the chief contributor but into the columns of which others gradually entered and which long before his death had assumed a dignity and acquired an importance which must have been a source of growing delight to him as the years wore on.

The decade preceding the monographic stage of his activity did not pass by without an occasional glimpse into the significance of things which in themselves were scarcely more than interesting; for example, the distinction between the great groups of Monocotyledons and Dicotyledons (1879), the development of a flower (1883), the question of floral "adhesions" as the older morphologists had regarded them (1885), pollen "spores" (a new expression in those days), and the development of the fruit of Umbelliferae (1887). The trend of his activities in this period are nowhere more clearly indicated than in the publication in 1886 of a "Handbook of Plant Dissection" of which he was a joint author, and in which, as in much of his later work, the influence is evident of Strasburger, Sachs' great successor as a leader.

It was at this stage of his evolution that, in preparing a vice-presidential address for the American Association for the Advancement of Science, he outlined his views on the future of that field of botany which to the end he regarded as his own, "systematic" botany. As he saw it then, and evidently continued to see it, three indispensable coordinated units enter into this field, "equally important and equally honorable"; collection and description of the kinds of plants which make up the vege-

tation of the earth; a study of their life histories (the branch of morphology into which he was entering); and the construction of a truly natural system of classifying them.

The ten years following Coulter's resumption of active botanical teaching, at the University of Chicago, in the prime of an experienced and reflective middle age, for he was then 45, were those of his most important professional career.

In this period, realizing the fundamental taxonomic importance of ontogenetic morphology, his original publications are concerned with such topics as chalazogamy, fertilization, heterospory, gametophytes, and embryogeny. This, and the next decade and a half is the period in which appeared the "Morphology" textbooks of Coulter and his talented pupil and associate, Chamberlain, a series covering comprehensively and with various revisions both great groups of the Spermatophytes—in no small way a summation of the individual researches of the many men whose pride and joy it is to have the dissertations in their candidacy for the doctorate marked with his signature.

Coulter's place as an American teacher of botany falls in that time when he aligned himself with two men, somewhat his seniors, Beal and Bessey, in popularizing in this country the "new botany" (as one of them called it) that the masterly text book of Sachs had presented so alluringly. Of the three, he stands out preeminently as a productive investigator; with them he shares the distinction of having trained excellent workers as well as teachers; and no colleague in the United States has approved as teacher the credentials of so many recipients of the philosophy doctorate, which stamps—if it means anything—the trained workman in productive scholarship.

An admirable lecturer, from time to time he crystallized in printed form the clean-cut impressions that constituted the charm of his oral presentation before a class, and his readable texts reflect concisely and clearly the knowledge of their day in the fields that they cover.

For Coulter, environment and attendant opportunity changed, especially in the latter respect and for the better with the years. For five years, beginning with 1874, he was Professor of Natural

Sciences in Hanover College; for a dozen years following, he was Professor of Biology in Wabash College. Neither of these excellent but small colleges was largely equipped with library or laboratory facilities for more than rudimentary work, nor did either pay salaries permitting private expenditure for professional purposes beyond very narrow limits; but both were ideally located in an attractive country and in very humanly livable small communities. It was here that Coulter developed as a systematist; and it was during this period that he appears to have made the largest number of directly personal observations on nature.

As too often happens with versatile and presentable men, this period of rather broadly conceived professorships was followed by five years of administrative work; as President and at the same time Professor of Botany at the University of Indiana for two years, and as President of Lake Forest University for three years. It is greatly to his credit that he found time from executive duties during these years to do much of his best monographic work.

Like the great geologist, Chamberlin, Coulter very gladly relinquished the dignity and burdens of a presidency to head a department in the newly organized University of Chicago, and he continued in this capacity from 1896 until 1925, when, almost at the age of 74, he retired from active teaching though not from professional activity, for his remaining years were spent as Dean and chief scientific advisor of the Boyce Thompson Institute for Plant Research at Yonkers, an establishment in the inception of which he had exerted large influence and as the active head of which he had placed one of his most capable graduates. It is at once a characteristic and a tribute to the success and judgment of Coulter, that for the school of botany which he created in Chicago, as for this latest fruit of his talent, he did not find it necessary to turn to others than his own former students when filling botanical positions.

As editor of the *Botanical Gazette* for half a century, Coulter was called on to appreciate and to chronicle the passing of many botanists of eminence. These biographic and bibliographic

notices were usually sympathetic and always to the point and well phrased. Quite apart from textbooks and the papers and books embodying the results of his own studies and those of his students and colleagues, for he liberally used the discoveries made by sharp eyes in what would otherwise have been perfunctory and scientifically fruitless advanced classwork—Coulter spoke often and wrote voluminously, on a large range of educational and humanitarian subjects. Among these, in his later years, organic evolution stood well to the fore; and the solid support afforded by all knowledge of nature to rational religion was never far from his mind and precept.

No account of John Coulter's personality and activities would be at all balanced if it stopped short with the recital of his influence and personal achievements in the science to which his life was nominally devoted. Though he seems not to have taken hold, in a conspicuous way, of civic movements for material betterment of the communities in which he lived, he was an active worker, and often a leader, from start to finish in movements for their human uplift.

Though never a seeker after such connections, Coulter was counted as an honored and helpful member of various organizations connected with his scientific activities, as, for example, the American Association of University Professors, the Indiana, Illinois and Chicago Academies of Science, the Botanical Society of America, and the American Association for the Advancement of Science, over each of which he presided in due course. He was elected to the National Academy of Sciences in 1909.

In no botanical association did he enjoy greater affection than in his religious fellowships. Seeing nature and its mysteries and wonders through the eyes and with the understanding of a naturalist, he saw the Author of nature through the eyes and with the faith of a Christian. For many years he led a young men's class in his church every Sunday morning when it was physically possible for him to meet with his class—and as he handled his affairs few obstacles arose that kept him from this which he regarded as the greatest of the week's privileges and duties; and his religious influence on the campus was very great.

The name of John Merle Coulter is commemorated in two genera of flowering plants, *Coulterella*, Vasey and Rose, and *Coulterophytum*, Robinson; and it cannot be forgotten at the University of Chicago so long as the Coulter Research Fellowship in his chosen field endures, for one of the last joys of his life was that of knowing that funds had been raised and accepted for the permanent endowment of such a fellowship.

Coulter is likely to be remembered long as a botanist, longer as a teacher of botany, and longest as a kindly, friendly, good and honest man of the highest ideals and possessed of the talent for inoculating others with them; but he is and will be held in most loving memory by those who knew him in his family life and to whom he was the spirit and personification of home.

The accompanying list of Coulter's publications is from the pen of his long-time associate and friend, Professor J. C. Arthur, of Purdue University.

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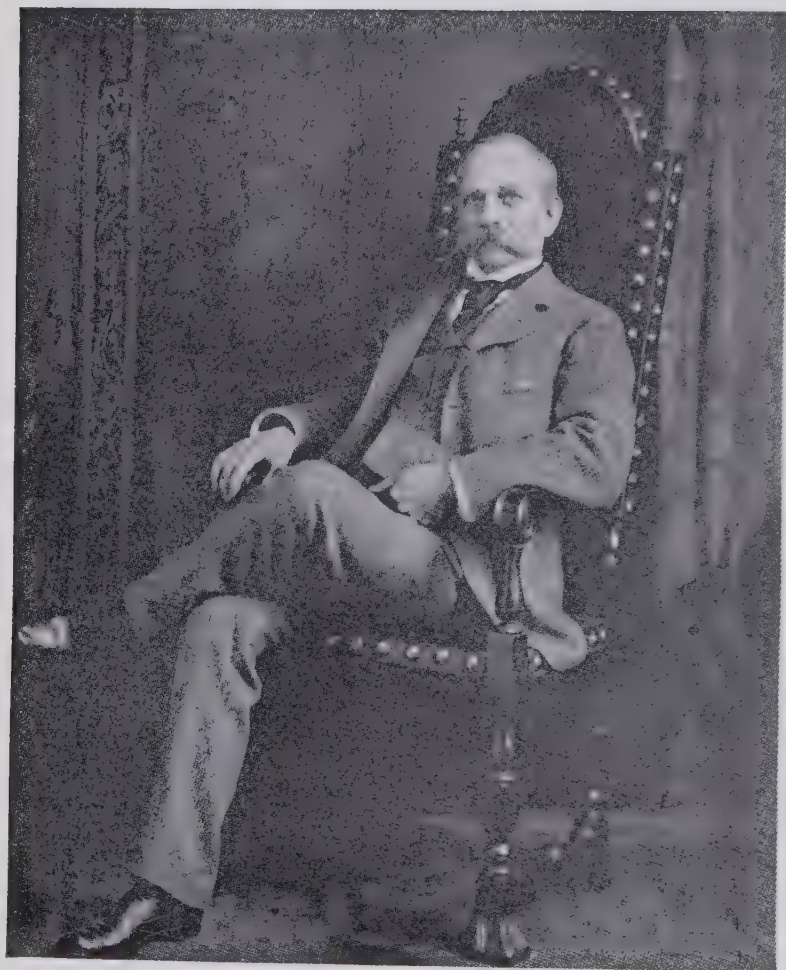
CHARLES FREDERICK CHANDLER

1836-1925

BY

MARSTON TAYLOR BOGERT

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1930



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CHARLES FREDERICK CHANDLER

BY MARSTON TAYLOR BOGERT

I. ANCESTRY

The first Chandlers to arrive in this country were William Chandler and Annis, his wife, who came to Roxbury, Massachusetts, from England, in 1637.

On the paternal side, the Chandler ancestors were prominent in Massachusetts life, three successive John Chandlers having been Judges of Probate in Worcester County and Colonels in the Provincial Army. The Revolution broke out during the life of the last of these three, who begged to be permitted to remain neutral, as he certainly could not bear arms against his fellow-countrymen nor could he bring himself to fight against the flag under which he and his forebears had fought. But feeling ran so high that he and eight other royalists were expelled from the country and their property confiscated. This John Chandler settled in London where he lived until 1800, and where he was generally known as "the honest refugee," because he never uttered any complaint nor made any demand for reimbursement for the financial losses suffered. At his own urgent request, his wife and children had remained in America, and one of these children, Nathaniel, who graduated from Harvard College in 1792, was the grandfather of Charles Frederick. It was in this grandfather's home, in Lancaster, Massachusetts, on December 6, 1836, that Charles Frederick was born.

His maternal grandfather was John Whitney, an old Boston merchant, and his grandmother was a daughter of John Slack.

II. BOYHOOD

His father, Charles Chandler, owned a dry-goods store in New Bedford, Massachusetts, which provided a modest living for the family. Their home was located at the corner of Third and Bush Streets, and there Charles passed his early life, in company with his sisters, Catherine and Mary, and his brother, William H., later Professor of Chemistry and President of Lehigh University.

As a boy he attended the New Bedford High School, where he received his first lessons in chemistry. The teacher in this subject, fortunately for the cause of that science, was a young and gifted instructor, Hemingway by name, whose fascinating and inspiring manner of presenting the subject made a wonderfully deep and abiding impression upon the eager plastic mind of the boy. It was the genius of this teacher which planted the seeds that quickly sprouted and grew until chemistry became the over-spreading interest of young Chandler's life.

At about this time, his father was having some difficulty in finding a boy to help in the store, so Charlie volunteered for the job. To his father's objection that it would interfere with his school work, the lad replied that he could get up earlier in the mornings and work after hours in the afternoons. Former store boys had received a dollar a week. Charlie agreed to take the job for fifty cents per week and one cent per package delivered. He arose in the mornings at six o'clock, swept the store, attended to the fires, trimmed and filled the lamps, and looked after the other chores until it was time to go to school. In this way, he earned enough money to build and equip a little chemical laboratory in the attic at home, and there he made olefiant gas from alcohol and carried out other simple chemical experiments, much to his own delight and to the admiration of his schoolmates. One of these experiments, carried out when he was but fourteen years old, was recalled by him a lifetime later when he appeared on the witness stand as chemical expert for John Wesley Hyatt in defense of the celluloid patents. An uncle of young Chandler, having failed in his attempts to make gun-cotton for his rifle, turned over the materials to his nephew, and the boy succeeded where the man had failed. In fact, he both startled and entertained his family and friends with demonstrations of the excellence of his product, for he would place a little of the material with a few grains of black powder on the palm of his hand; or on the parlor table, and then touch off the gun-cotton with a match. The cotton would flash and disappear, leaving the black powder unburned, and flesh or table uninjured. Another source of modest income was the

money received for taking care of the family vegetable garden, at so much per week, payable annually on July 4th.

On Sunday afternoons in the spring and early summer, when the weather was fine, his father often took the children out for a ramble in the woods, where they collected wild flowers and other natural history specimens; or they would go down to the seashore, at what was then known as Clark's Point, and, with a cloth bag tied to the end of a long pole, gather algæ which were brought home, there to be carefully floated in a basin of water, a sheet of paper slipped underneath and the specimen removed and mounted. In this way, an algal herbarium was gradually accumulated.

The summer vacations were spent at his grandfather's place in Lancaster, a locality noted for the variety of fine minerals to be found in the vicinity, such as chialstolites, petalite, spodumene and many others. The librarian of the Public Library there, himself an ardent mineralogist, aroused the boy's interest in this field of natural science and stimulated him to search for these and other rare minerals. The Lancaster Academy offered certain summer courses, and here, when only ten years old, the youngster took his first course in botany, under Miss Emily Shaw, the head teacher, using as textbook Miss Lincoln's "Botany."

The New Bedford whaling vessels often returned in ballast, and not infrequently this ballast contained minerals not found in the neighborhood of their home port, such as flints, geodes and others, and from this source also the boy gleaned for his collection.

At that time, the elder Agassiz used to journey down from Harvard to New Bedford occasionally, to lecture before the Lyceum there on corals, fishes and other zoological topics, and when he had arrived at the advanced age of fourteen, young Chandler was allowed to attend these lectures. The eminence of the lecturer, and the absorbingly interesting way in which the subject was presented, charmed and enthralled the budding scientist in the audience and further fired his imagination with the wonderful opportunities for science in the service of mankind.

After graduating from the New Bedford High School, he continued his studies privately with a friend of the family for nearly a year, to make up the Latin and Greek required for admission to college and which had been neglected in his eagerness for science, and then set out in the autumn of 1853 to secure an education in chemistry.

III. STUDENT DAYS AT COLLEGE AND UNIVERSITY

In those early days, there were but few places in the United States where even an elementary training in this science was obtainable. Chemical laboratories had been established at Yale College and at Amherst. There was a private laboratory in Philadelphia, and one had just been equipped and opened in the Lawrence Scientific School of Harvard College. Chandler decided upon the last as being nearest to his home, and in September, 1853, began the study of his chosen profession under the genial Professor Horsford, who was there only on part time. Quite naturally, he attended also the lectures in geology by Agassiz, whose addresses at New Bedford he remembered with such keen delight.

There were about a dozen students in the chemical laboratory, half of whom were beginners. The textbook used was Will's "Qualitative Analysis," and all the students were set to work on the analysis of the "100 bottles." When this task was completed, Quantitative Analysis was undertaken. The chief difficulty encountered, however, and one which was then quite general throughout the United States, was that there had never been worked out any carefully planned and well balanced curriculum, of a definite number of years, for the training of those who wished to be professional chemists. In fact, at that period there were no lectures on chemistry at all for the students of the Scientific School at Harvard, and such students were not generally permitted to attend the lectures in this subject given by Professor Josiah Cooke to the College students, although young Chandler succeeded in hearing a few of these. They were expected to provide themselves with suitable textbooks and study these at home, while at the Scientific School they were

simply turned loose in the laboratory and supposed to look after themselves.

The inevitable result of this condition of affairs was the exodus to Europe of increasing numbers of the abler and more ambitious American students, the majority of them going to Germany.

While Chandler was at work in the laboratory one day, Charles A. Joy, Professor of Chemistry at Union College, who happened to be visiting Harvard, came in and recounted to the boys some of his experiences as a student at Göttingen and Berlin. With characteristic promptness, Chandler sat down that evening and penned an epistle to Professor Joy telling him that there were a number of the young fellows at the Lawrence Scientific School who were eager to become chemists and would like his advice as to the best way of achieving their ambition. Within a few days he received a lengthy response from Professor Joy, advising him to go to Germany if possible and giving all necessary information as to details.

After a conference with his uncle, Professor Theophilus Parsons of the Harvard Law School, with Professor Cooke and others, as well as with his parents, the latter agreed to send him over, for they wished their boy to have the best education the world could give, although it meant a heavy struggle on their part and the sacrifice of many personal comforts to raise the money required. Fortunately, a friendly neighbor who owned some whaling vessels heard of the contemplated trip and told young Chandler that he had a vessel which would be sailing in a few days for Antwerp with a cargo of whale oil and other freight and that if he wished to go over on her, it would not cost him a penny; and in this way he took his passage across the Atlantic in 1854, without expense and the only passenger on board.

Arrived in Europe, he directed his steps first towards the University of Göttingen. In response to the young man's request, Professor Joy had given him a letter of introduction to Professor Friedrich Wöhler, and Chandler had inquired, with characteristic thoughtfulness and Yankee shrewdness whether he might not supplement this by some gift or offering in recog-

nition of the courtesy he was asking. Joy, knowing Wöhler's love of rare minerals, suggested that he take over with him a few of the less common ones available here and, with the help of his good friend the Lancaster librarian, he gathered a collection of specimens which in those days were very difficultly obtainable in Germany. Wöhler was so delighted, not only with these rare minerals, but with the personal attractiveness and evident earnestness of their bearer, that he took him immediately into his private laboratory where he labored, in the words of this master, "mit meisterhaftem Fleisse," and where he found conditions much more favorable for the study of chemistry as a profession. There were thirty students in Wöhler's laboratory then, thirteen of whom were Americans, and they had the opportunity of attending also lectures in physics by Weber, in botany by von Griesbach, and in mineralogy by von Waltershausen.

In the fall of 1855, he moved to the University of Berlin where, through the influence of Wöhler and his friend, Professor Joy, of Union College, he was fortunate enough to secure the position of private assistant to Professor Heinrich Rose, in whose laboratory his only companion, besides Rose's lecture assistant, Oesten, was Nils Erich Nordenskjöld, later famous as an arctic explorer. He attended the lectures of Heinrich's brother, Gustav Rose, on mineralogy, of Magnus on industrial chemistry, and of Dove on physics.

It was while he was a student at Berlin that Alexander von Humboldt invited him to the Royal Palace at Charlottenburg, where he himself was a guest, and became so interested in the young man that he spent an hour and a half talking to him.

In 1856, he received the degrees of A. M. and Ph. D. from the University of Göttingen. His dissertation was entitled "Miscellaneous Chemical Researches," and was reprinted (in English) at Albany, New York, in 1857, by C. Van Benthuyssen. It was dedicated to his father, Charles Chandler, "As a slight tribute of grateful affection," and consisted of analyses of the following rare minerals: I. Zircon from Buncombe County (North Carolina), II. Saussurite from Zobten, III. Stassfurthite from Stassfurth, IV. Analysis of a rock resem-

bling talcose slate, from Zipser, V. Columbite from Middletown (Connecticut), VI. Columbite from Bodenmais, VII. Tantalite from Chanteloube, VIII. Yttrotantalite from Ytterby, and IX. Samarskite from the Urals. All of these analyses were carried out in the laboratory of Professor Heinrich Rose at the University of Berlin. In addition to these analyses, the dissertation contained also X. Experiments on the cerium metals, and XI. Artificial heavy spar, the work on which was conducted in the laboratory of the University of Göttingen under the supervision of Professor Friedrich Wöhler. Bearing in mind this early training, it is not surprising to find that Chandler maintained throughout his life a keen appreciation of the importance of analytical chemistry and a strong predilection for it, as well as a very lively interest in minerals and mineralogy.

Thus having equipped himself with the best training which the world could provide at that time for those who wished to make chemistry their life's work, he turned his face once more towards the homeland, reaching our shores in the autumn of 1856, where he soon discovered that interest in the subject of chemical education was already increasing and that additional colleges and universities were opening chemical laboratories for practical instruction.

He sought to establish himself in New Bedford as an expert in oil chemistry, but soon discovered that while the whale oil industry occasionally required his services for the analysis of their products, there was not enough business to provide a living and no great promise for the future in that line.

IV. AS TEACHER

1. At Union College, Schenectady, New York

Having heard that his old friend, Professor Joy, of Union College, needed an assistant, Chandler proceeded at once to Schenectady, only to find that there was no budget appropriation for an Assistant in Chemistry, but that there was an item of \$400 for janitorial assistance. Characteristically undaunted, Chandler accepted the job of assistant, with the official rank

of "janitor," and for this munificent salary the new incumbent, in addition to his teaching duties, actually swept and cleaned the laboratory, made the fires, carried the fuel and ashes, and did all the other chores.

It was in January, 1857, that he accepted this job of Janitor-Assistant in Analytical Chemistry on the staff of Professor Joy, and in the spring of that year they opened the new chemical laboratory, with an enrollment of six students in the first class. At that time, Union was one of the largest colleges in the United States, ranking even Harvard in the number of its students.

In April of the same year, Professor Joy was called to the Chair of Chemistry at Columbia College, and Chandler succeeded him at Union, lecturing to 150 seniors, most of whom were older than their lecturer. The late U. S. Senator Warner Miller, of Herkimer, New York, was a member of that class.

Little attention had been paid at Union to instruction in the sciences, so that permission was secured without difficulty for the introduction of new courses from time to time, with the result that he was soon lecturing in general, inorganic, organic, analytical, and agricultural chemistry; blowpipe analysis, mineralogy and geology.

For more than seven years he remained at Union in charge of the lecture and laboratory work in chemistry and other subjects, first as Adjunct, then as full Professor of Chemistry, rapidly building up a strong and influential scientific branch of the college's work.

It was during this period that he collaborated with Dr. D. K. Tuttle of the University of Virginia in the preparation and publication of "A Manual of Qualitative Analysis," which appeared in 1860, and was reprinted by John Wiley & Son, of New York, in 1873. It bore the following dedication: "To Friedrich Wöhler, Professor of Chemistry in the University of Göttingen, this little work is respectfully inscribed by his pupils, the Authors." It employed the old dualistic chemical formulas (e. g. PbO , NO_5 for nitrate of lead, HO for water, et cetera) and was quite innocent of chemical equations of any kind or of theoretical discussions. It was a clear and compact

presentation of the more important analytical tests for the detection and separation of the principal bases and acids, arranged in the analytical groups common to that day, and was doubtless very helpful to the students as a laboratory *Vade Mecum*.

His skill and power as lecturer and teacher were manifest even in those early years and his personal popularity was attested by his election to the Kappa Alpha Fraternity.

Union College possessed an unusually fine collection of minerals which had been brought together by Mr. Charles M. Wheatley of the Wheatley and Perkiomen Mines of Pennsylvania, and had been purchased for the college by Mr. Delavan, owner of the Delavan House, at Albany, N. Y., for \$10,000. *

This splendid collection was the lodestone which attracted to Schenectady scientists from all parts of the country, among them Professors Benjamin Silliman the elder and Brush, of Yale, as well as Dr. Thomas Egleston, a mining engineer, of New York, who had received his early training under Professor Silliman and then had studied at the *Ecole des Mines* of Paris.

It was in 1864 that Egleston's visit occurred and that he made the acquaintance of Chandler and began a friendship which lasted until his death. He was somewhat startled to see Chandler conducting commercial assays of ores for silver and gold, and to learn that he gave lectures on the subject also, for he had been under the impression that only the graduates of foreign schools of mines were competent to give instruction in that field. This was one of the considerations which led Dr. Egleston to the decision that Chandler was the man needed to join with General Vinton and himself in the project of establishing a School of Mines in New York City, as a part of Columbia College, and a formal invitation arrived soon afterward.

Chandler was disposed to accept, but many of his friends attempted to dissuade him, arguing that he would be leaving a good position with a promising future and a friendly Board of Trustees, for what was at best an uncertainty, both as to its present success and its future prospects, and as concerned the

attitude of the Columbia Trustees toward him and the undertaking. But, as already mentioned, his old friend, Professor Joy, was now a member of the Faculty of the School of Arts, or academic department of Columbia, and naturally he was one of the first with whom Chandler discussed the matter, to learn how Joy would regard his coming to Columbia and to prevent the birth of future misunderstandings and possible friction. Joy readily admitted the possibility of developing at Columbia the kind of school which Chandler and his associates had in mind, but was quite frank in acknowledging the fear that the presence of two professors of chemistry at Columbia might lead to some conflict of authority, overlapping of work, or disturbance of the existing courses in chemistry, "unless," he added to Chandler, "that other professor were you, in which case I would have no fear of such interference."

Thus reassured, Chandler accepted the appointment, and he and Joy worked together in friendship and harmony until the death of the latter resulted in the transfer of his courses also to Chandler.

To the credit of Chandler's deep sense of patriotic duty be it said that he delayed his departure from Schenectady until after Election Day, 1864, so that he might cast his vote for Abraham Lincoln.

In these days of prohibition agitation, it is worth noting that one of his earliest contributions was "An investigation on the formation of alcohol during fermentation," which was published in "Biblical Temperance," by E. C. Delavan, Esq.

The influence of his training in Germany for his Ph. D. degree was manifested in the immediately succeeding years by his continued attraction to the field of analytical chemistry while he remained at Union, during which period he published the following papers:

Analysis of Dolomite. In the Report of the Geological Survey of Iowa, by James Hall and J. D. Whitney; Albany, 1858.

Examination of interesting urinary calculi, included in a report of Dr. Alden March. Printed in the Annual Report of the New York State Medical Society for 1858.

"Analysis of Datolith." *Am. Jour. Sci.*, 28, 13 (1859).

"A Manual of Qualitative Analysis," by D. K. Tuttle, Ph. D., and C. F. Chandler, Ph. D., 1860; reprinted in 1873 by John Wiley & Son, New York.

A new metal in the native platinum of Rogue River, Oregon; *Am. Jour. Sci.*, 1862, 351.

Analyses of one blende, two smithsonites, one cerusite, and with J. P. Kimball, analyses of nine shales, five galenas and one dolomite; in the Report of the Geological Survey of the Upper Mississippi Lead Region, by Professor J. D. Whitney, Albany, 1862.

2. At Columbia University, New York, New York

School of Mines.—Fortunately for the School of Mines project, Columbia had just installed, in 1864, Dr. Frederick Augustus P. Barnard as its president. Himself a distinguished scientist and one of the founders of the National Academy of Sciences, President Barnard immediately welcomed the idea of such a scientific school for Columbia and followed with sympathy and understanding the growth and development of the venture.

At the time of Dr. Chandler's move from Union to Columbia College, there were but few technical schools in the country and no mining schools, and many regarded this project of founding a School of Mines in New York City as decidedly visionary and unpromising. Nevertheless, the three professors immediately concerned were so enthusiastic on the subject and had such unbounded faith in its ultimate success, that they agreed to undertake the work without any definite or guaranteed salary whatever, other than the fees received from such students as might register for the course. The timid and those without vision regarded this as very foolish on Chandler's part, for he was now married, and there seemed little likelihood that his share of the students' fees would be sufficient in amount to support a family in New York.

George T. Strong, William E. Dodge, Jr., and several other good friends provided a total of about \$5000 to equip the laboratories. A fine cabinet of minerals was donated by Gouverneur Kemble, and President Barnard, Dr. Torrey and other

Columbia trustees encouraged the enterprise in every way possible.

Some vacant rooms in the basement of the old college building on Madison Avenue and 49th Street were fitted up as laboratories and accommodations there provided for twelve students. The new school opened its doors on November 15, 1864, and twenty-four students presented themselves for admission on that date. Chandler often told, in his own inimitable way, how he used to open the laboratory at seven A. M., start the fire to warm it up, sweep and clean the room, and do all in his power to make the place as attractive as possible and the lot of the students a pleasant one. It was inevitable that there was passed on to these students at the same time some of his own overflowing energy and enthusiasm.

The school was a phenomenal success practically from the beginning. During the entire winter, carpenters and plumbers were kept busy installing new desks for new students and the number of students for the first year finally rose to forty-seven. During the following vacation a large four-story factory building on the campus, formerly used for the manufacture of sash and blinds, was placed at the disposal of the school, and sufficient funds provided to equip it with suitable laboratories, lecture rooms, et cetera. Accommodations were arranged for seventy-two students. Eighty-nine registered for the second year and the school was thronged. The success of the venture being so great and so obvious, the trustees finally arranged to place it on a substantial basis as an integral part of the college work. Professor J. S. Newberry was called to the chair of geology, relieving Chandler of this subject, and a complete faculty of professors and junior officers was established. A new building was erected especially for the school, the plans for which were drawn up by Dr. Chandler himself. It was designed to accommodate one hundred and fifty students and was soon overflowing.

The school continued to prosper and its field to unfold and expand until what began as the School of Mines in a few basement rooms with twenty-four students, had become at the time of Chandler's death a whole group of great technical

schools (Mines, Engineering, Chemistry, Architecture and Pure Science) with many thousands of students and six splendid buildings and with a reputation not merely national, but international.

Although at first appointed Professor of Geology, Analytical and Applied Chemistry and Assaying, Dr. Chandler soon was made Dean of the school and became its leading figure, all-pervading genius and chief driving force. The school had its own Bursar (Mr. Fisher) and its own Registrar, both of whom served under the immediate personal supervision of Dean Chandler and formed part of his office staff. For thirty-three years he served as Dean, and of the many able and devoted men who built their lives into this splendid institution, none contributed so much as he. In 1897, when the University moved to its new site upon Morningside Heights, Professor Chandler resigned his post as Dean, but continued in charge of the Department of Chemistry as the "Mitchill Professor of Chemistry" until his retirement from active service, July 1, 1911. In the early days, he lectured upon all branches of chemistry, both theoretical and applied, but as the staff grew in numbers he transferred to his younger associates most of this so that during the later years of his professorate his lectures were mainly upon general and industrial chemistry.

It was largely through Dr. Chandler's personal solicitation and influence that the Havemeyer family generously provided the funds for the erection of the splendid chemical building which bears their name. Before drawing up the plans for this building, Dr. Chandler made a special trip to Europe for the purpose of examining the construction of the leading chemical laboratories there, with the result that Havemeyer Hall, when finally completed, embodied the best that was then known concerning laboratory construction, and was universally regarded as the finest chemical laboratory in the United States.

Under date of January 3, 1910, Professor Chandler addressed the following communication to the President of Columbia University:

"MY DEAR DR. BUTLER:

"After mature deliberation I have decided that it is much better to withdraw from active service while one is in the full enjoyment of health and strength, rather than to wait until the infirmities of age make it evident to all that one has outlived his usefulness.

"This is my forty-sixth year of service at Columbia, and my fifty-fourth year of college teaching, and I feel that I have had my fair share of this most agreeable life.

"I wish to take this opportunity to express my gratitude to you and the other members of the Board of Trustees and their predecessors for the confidence with which they charged me from the outset with the various duties of instructor, dean, bursar and registrar, and the generous support which I have always received from them.

"I would respectfully request that I may be relieved from the active duties of my professorship at the end of the present academic year.

"Very respectfully yours,

"CHARLES F. CHANDLER."

The minute adopted by the Trustees in accepting this resignation, after reciting briefly his career, concludes with the following: "Professor Chandler will carry with him into his retirement the affectionate regard and esteem of two generations of students as well as a host of colleagues on the teaching staff of the University. The Trustees record their grateful appreciation of this long and generous career of devoted service."

He was granted a year's leave of absence on full pay, and appointed Emeritus Professor of Chemistry to take effect upon the date of his retirement, July 1, 1911.

The Columbia Alumni established a Chandler Lectureship and Chandler Medal at Columbia, and gave a huge banquet in his honor. On that occasion, President Butler spoke with deep feeling of Dr. Chandler's approaching retirement and paid a glowing tribute to the man and his work. "In the University," said he, "as in the nation, we mark most of all, and we cherish beyond all else, the coming and the passing of personalities.

No substitute has been found in specialized scholarship or in technical skill or in acquired art for those precious qualities of mind and heart that make a man a great teacher to generations of students. . . . It is by the coming and the passing of the services of these personalities that we mark the real history of Columbia, and we shall be poor indeed, no matter how magnificent our site, how splendid and how numerous our buildings, how large our endowments and how great our enrollment, we shall be poor and wretched indeed when personalities—great human personalities—are no longer found in Columbia's life. . . . The world is full of chemists, but there are not very many Chandlers. . . . I know that we cannot get on without personality, and I mark with sadness and sorrow the retirement from active service of a personality which has been familiar to me for thirty-two years and which has always been full of charm, abounding in loyalty, generous in doing for Columbia and the public, and anxious to find new ways in which to serve. I call that a great academic and a great human service and am glad to have had this personality on the proud rolls of Columbia."

President Butler's Annual Report of November 7, 1910, contains the following additional reference to Dr. Chandler: "To his teaching power as well as to his effective and conscientious service as administrator, the Department of Chemistry and the School of Mines, to which it primarily belonged, owed almost everything for many years. Professor Chandler has long been a point of contact between the University and the public, between science and industry and the public health. His career is unique of its kind, and we shall not soon look upon his like again."

In commenting upon the same subject, the Columbia Jester, a student publication, in its issue of January 20, 1910, has this to say as indicative of the students' attitude: "Year after year he has taken the entering classes by the hand and has led them through a course of Chemistry, Ethics and Humor, so cleverly combined that it has made men of them. It is a striking tribute to his popularity that the class never fails to stamp their ap-

plause when he enters the room. It is a striking tribute to his splendid, practical ideas of health and to his wholesome, hearty, jovial nature that he is still only a boy, and with a mind and body sound enough to do all that a boy can do. . . . We who have had the last chance to see him and hear him in the midst of the labors which were truly play to him, should be both proud and happy that this opportunity has not been missed. Truly do we fear for the future Freshmen who will not have him as a guiding spirit. And though we lament that so intimate and cherished a connection is finally to be severed, we rejoice that our last picture of him is one of a man hale and hearty, in the full possession of all his faculties and talents, working up to the last moment at his life's profession, and yielding finally to younger hands so that he may spend his remaining years as overseer of the work he carried to so high a development."

I first became acquainted with Professor Chandler in 1888 when, as a student in the Columbia College School of Arts, I attended his lectures in general chemistry.

The College then occupied the block bounded on the west by Madison Avenue, on the north by Fiftieth Street, on the east by the tracks of the New York Central and New York, New Haven and Hartford Railroads, and on the south by Forty-ninth Street. Across the railroad tracks were the buildings and grounds of the Woman's Hospital. Across Forty-ninth Street was "Fritz's" bar and lunchroom. At Forty-ninth Street and Fifth Avenue was the Hotel Buckingham, familiarly known as the "Buck," a favorite hang-out for the students.

The Forty-ninth Street front of the College block was occupied by Hamilton Hall, containing the offices and classrooms of the academic department, or School of Arts, as it was called, with the office of President Barnard on the second floor, and the students' locker-room in the basement, presided over by that ubiquitous and kindly old proctor, Stephen Weeks. Columbia students of more than one generation will recall "Stevie," with his dark suit and plug hat, his spare figure stooping somewhat in the later years of his long service, as he moved

from place to place to see that proper order and discipline were maintained among the students.

On the Forty-ninth Street side was the new library building, where Melville Dewey, of "Dewey-decimal" fame, held sway, until he withdrew and was succeeded by Librarian Baker.

The old asylum building, popularly known as the "Maison de Punk," occupied part of the Fiftieth Street front just north of the library. Adjoining the Maison de Punk on the east was another old building, the upper floor of which was used for the Chapel; Rev. Mr. Duffy was Chaplain at the time. This old building was so close to the library that at one place the passage between the two was so narrow students could go through only in single file. It was therefore dubbed the "Pass of Thermopylæ" and many were the scrimmages staged there.

The School of Mines building filled the northeast corner of the block. It was L-shaped, one leg running along Fiftieth Street and the other along the railroad tracks on the east side of the block. Inside of this angle formed by the two wings of the School of Mines building the Power House was situated, and south of it, on the Forty-ninth Street front, between the east end of the library and the south end of the east wing of the School of Mines building, was the Administration Building, where were the offices of the Superintendent of Buildings and Grounds and others. A footpath ran from Forty-ninth Street straight up to the main door of the School of Mines building, and morning and evening Dr. Chandler could be seen going in or coming out, with his ever-present market basket on his arm.

Dr. Chandler was then Dean of the School of Mines, as well as its Professor of Chemistry. His office was on the ground floor just to the left of the entrance. Here his faithful aid, George Fisher, performed for many years the duties of Registrar and Bursar.

Most of Chandler's lectures were given in the large lecture room on the ground floor at the south end of the east wing of the building. In these lectures he was assisted by Dr. Louis H. Laudy, his "fidus Achates." Dr. Laudy was associated with him in the work for so many years that he became a sort

of tradition. Few knew just when he first entered upon these duties, so that it was generally said that Chandler must have graduated "summa cum Laudy."

Like most great teachers, Dr. Chandler had a keen sense of humor and his lectures were always enlivened by witty stories, timely jests, waggishness, puns and amusing anecdotes.

When discussing photographic and photo-mechanical processes, on one occasion, he produced a picture of a very attractive young lady, which he said was Titian's daughter, Polly, claimed to be the first politician.

Dr. Edward Gudeman of Chicago, who was his assistant for a time, recalls the following. Dr. Chandler, during his lectures upon specific gravity, was endeavoring to bring home to his class the fact that substances lighter than water not only would float but also possessed a buoying capacity equal to the difference between their weight and that of an equal volume of water. To illustrate, he recalled the case of the Baptist minister who was considerably embarrassed by his lack of success in trying to immerse completely an elderly and somewhat corpulent sister, until he learned from one of his elders, who was the physician of the lady in question, that she was buoyed up by gas in the stomach and a wooden leg.

Once when called upon unexpectedly by the chairman of a meeting, he was enjoined to speak with telegraphic brevity. To which he replied that while the body of the telegram was usually ten words, the address was always unlimited.

On another occasion he was being crossexamined in a lawsuit by a self-confident and very obese lawyer who, wishing to impress his man at the outset, said to him that whenever he was expected to examine a so-called chemical expert he always took the precaution of eating one before breakfast. "Which explains," said Dr. Chandler, "why you appear to have more brains in your stomach than in your head." After which encounter, the legal luminary handled the chemist somewhat more gingerly.

The late Senator Roscoe Conklin, who frequently stood with his hands in his pockets, introduced Dr. Chandler at a certain meeting as that "*rara avis*—a doctor who takes his own medi-

cine." Dr. Chandler retorted that he was proud to be introduced "by that eighth wonder of the world—a lawyer with his hands in his own pockets."

To illustrate the difference between politeness and tact, he used to enjoy telling the story of the week-end guest who, finding the bathroom door unlocked, started to enter, when he suddenly saw his hostess about to step into her bath. Backing out hurriedly, he said, "Excuse me, sir," The "Excuse me," said Chandler, was politeness, but the "sir" was tact.

Dr. Ellwood Hendrick tells the following amusing anecdote: "Dr. Chandler was an intimate of the late Henry H. Rogers, the guiding spirit of the Standard Oil Company. Wishing to play a joke on the doctor, Mr. Rogers once sent him a common towel which had been stained with greased ink, requesting him to remove that 'damned spot.' The request was accompanied by a long letter, which rehearsed the doctor's attainments, crediting him with every scientific achievement of the century, not only eulogizing his supremacy as textile, oil and color expert, but citing him as the only living authority in this field. It was requested that the work be completed and reported on within twenty-four hours. Dr. Chandler turned the matter over to his assistant with the suggestion that a pair of scissors might do the work.

"In twenty-four hours Mr. Rogers received the towel, the spot having been cut out and enclosed with it, and a detailed report which called attention to the marvelous rapidity with which the work had been executed, the monumental dexterity that had not only rid the towel of the blemish but had also preserved the spot intact. In view of this, it was stated, the bill enclosed was most reasonable, including only the cost of chemicals and apparatus employed, time and service not being charged for. The assistant signed the letter with the doctor's name, adding his own initials. In a few days the assistant was summoned to the professor's office to receive a cheque for a respectable sum of money from Mr. Rogers, with a letter which said that the assistant and not the spot should be damned. Later on he remarked that Professor Chandler trained his assistants well."

As an educator, Dr. Chandler possessed all the finer attributes of the great teacher. Imbued with a deep sense of his responsibilities to his day and generation, a keen appreciation and penetrating vision of the services which the scientist can contribute to the progress of civilization; self-forgetful and generous to a fault; with an abiding love for his fellow-man, an ability to see and understand the other's point of view, and a spirit of justice and fair dealing to which no one ever appealed in vain; he had a remarkable gift of transmitting across the lecture table to his students unconsciously, without preachment or pedantry, his own high ideals of the rôle of the scientist and the citizen. No one ever attended his lectures without recognizing the beneficent influence that radiated from this unusual personality. The student received not only an enticing introduction to the fascinating field of chemistry, but had before him constantly the potent example of a life really worth while and what that signified.

Like all great teachers, Dr. Chandler was an enthusiast for his subject. To him nothing was more wonderful, more beautiful, or offered greater opportunities for useful service than his beloved science of chemistry, and until his death he never wearied of talking about it or of listening to others who were discussing it. Time and again, in the later years of his life, I have heard him say to various gatherings of chemical students that what he envied them more than anything else was the many years they had ahead of them to devote to chemistry, and he would then proceed to picture to them, with so much fire and conviction, the delights of such a career, that they all went home with a new and heightened estimate of the dignity and importance of the work of the chemist.

His generosity to his students, particularly to those who found it difficult to meet the cost of tuition, was often remarked, but only those in his closest confidence actually knew how frequently these loans and advances were made. One instance will suffice for illustration.

I was standing by his side in his office one day some thirty years ago, and while he was opening his mail we were discussing matters of mutual interest. His correspondence was always

very heavy and as he opened the letters and spread them out on top of a pile which was already many inches high from the accumulations of previous days, he remarked that his letter pile was much like that of Robert Louis Stevenson, who referred to it as "a veritable quicksand, for anything placed on top of it slowly sinks down and disappears, never to come up again." As our conversation continued, he opened another letter and a slip of paper fluttered to the floor. I picked it up and handed it to him, whereupon he passed it back to me with the request that I examine it. It proved to be a check to his order for \$500 from someone I did not happen to know. In reply to my look of inquiry, he said, "That check is from one of my old boys. When he came here as a student many years ago, he had great difficulty in raising enough money to pay his fees, so I loaned him \$500. I heard from him occasionally after his graduation, but he was still having a struggle to make both ends meet and I came to the conclusion that there was little chance of the repayment of the loan. It is now several years since his last letter and I had about forgotten the incident, but if you will read this communication you will find that the writer says he is at last beginning to make his way and that the \$500 forwarded is the first that he has been able to save." How many other students were helped financially by Dr. Chandler probably will never be known, for he never kept any careful record of these accommodations and was the most lenient of creditors.

His generosity even went so far as to assist his university in the purchase of certain of the equipment for the new Havemeyer Hall laboratories. Part of this equipment consisted of many hundreds of sets of laboratory reagent bottles, with beautifully etched and painted labels which were imported from Germany. When these arrived, Dr. Chandler was much distressed to learn that the cost of Havemeyer Hall was exceeding the estimates and that there might be no funds available for the purchase of these laboratory reagent bottles. He was telling me about this trouble a day or two afterwards and said that he had decided to offer to pay for these bottles himself. "I have," he said, "five thousand dollars in bonds which I put

away for my old age, but I am offering them to the university in order that our laboratories may have this beautiful equipment." Whether Columbia accepted this sacrifice or not, I never learned, but it is to be hoped that it did not.

His services as chemical expert were in such demand that his contacts with chemical industry were numerous and often exceedingly important.

Largely as the result of this work, he accumulated a collection of chemical products of all kinds which grew so rapidly that it soon became necessary to house it in a separate room, and thus began Columbia's great Chemical Museum, which now bears the name of its founder. By purchase, as well as by gift, this collection was augmented until it was universally recognized as the largest and finest museum of the kind in the world, and a most valuable adjunct to the lecture courses. For many years it was unique, and it still remains one of the world's great collections. There the visitor will get a very vivid impression of the extent to which our modern civilization is dependent upon the science of chemistry.

With the death of its originator and patron saint, its growth ceased, partly because his successor as executive officer of the Department of Chemistry, Alexander Smith, was never greatly interested in it, partly because space and funds were not available for its development, and partly because changes in the organization of the work and in the methods of instruction rendered such a museum less necessary. Further, it has been found in several instances that small and highly specialized collections are often more useful when kept in association with the particular laboratories where such material is of immediate interest, than when maintained as part of one all-inclusive museum.

3. At the New York College of Pharmacy

Professor Chandler's educational activities were by no means limited to the Columbia School of Mines. Another New York institution which owes largely to him its present proud position is the New York College of Pharmacy.

This was organized March 18, 1829, at a meeting of leading pharmacists and wholesale druggists, but did not secure a charter until April 25, 1831. During its early years, it led a rather precarious existence and in 1866, Professor Peter W. Bedford of its faculty sought Dr. Chandler's assistance in the development of its work. At the time, the College occupied a single corner room in the second story of the old building of the University of the City of New York on Washington Square, and had an enrollment of thirty-two students. It is typical of the man that, in determining upon his answer, the only question he asked himself was whether or not it was to the interest of the community that he undertake it. It did not occur to him to inquire "What is there in it for me?" He said to himself, to quote his own words, "We must have apothecaries; we can't live without them, and they must be educated, for our lives are in their hands." He therefore accepted the invitation and lectured three evenings a week throughout the winter year after year, until some of these lectures could be transferred to new members of the staff. At first he received \$400, and provided apparatus and lecture material himself. The faculty then was composed of Professors Mayer, Bedford and Chandler.

In 1878, the College had outgrown its one room in New York University, so it purchased the old Morton Memorial Church at 209-211 East Twenty-third Street, and remodeled it to adapt it to the new needs. In these more commodious quarters the College grew so rapidly that in 1892 it purchased the plot at 115-119 West Sixty-eighth Street, and erected thereon the fine modern fire-proof structure which still stands as an enduring monument to the untiring zealous efforts of Dr. Chandler and his associates, Trustees and Faculty, and to the loyal support of the druggists of New York City. For many years, Dr. Chandler served not only as a member of the Faculty, but also as Vice President and finally as President, which latter position he was occupying when he received a letter from President Butler, in 1905, inviting the College of Pharmacy to become affiliated with Columbia University. This invitation was accepted by the College, and it has since been the College of Pharmacy of Colum-

bia University. Thus was justified the faith of those who in the days of small things labored so faithfully and self-sacrificingly.

That work of this kind is not without rewards which far transcend any financial remuneration is strikingly illustrated by the remarks of Dr. Chandler at a banquet tendered him by the officers, trustees, faculty and alumni of the College upon his retirement from active duty in 1910: "Wherever I see the green and red lights of the apothecary in New York, there I know I have a friend. I never go into a drug store but that some one comes up and tells me he was of the class of such and such a date, or is possibly a student still. Sometimes they are boys but newly matriculated at college. Sometimes they are old gray-haired men whose stooping shoulders and faltering footsteps make them seem older than I myself; always they are friends. This has been my highest reward, this has been my most cherished compensation. The feeling that I may have helped in the upbuilding of the institution, have aided in the formation of the characters of the rising generation of pharmacists and that this help has brought me the friendship of my students, is a source of pride and will remain a source of pleasure so long as I live."

4. At the College of Physicians and Surgeons

Another New York educational institution in whose development Dr. Chandler was enlisted not many years after his arrival in New York, and where he served with his customary distinction and success, was the New York College of Physicians and Surgeons, which later became the School of Medicine, or College of Physicians and Surgeons of Columbia University.

In 1872, he was appointed Adjunct Professor of Chemistry and Medical Jurisprudence under Professor St. John, and on the death of the latter in 1876 succeeded to his chair, lecturing on physics and chemistry every afternoon from 5 to 6 o'clock. This chair he held for over twenty years, retiring in 1897.

His voice was always raised there in favor of a more thorough scientific training for medical men and he was largely instrumental in having adopted the present four-years required course.

His work there, as was the case always, was progressive and constructive, and this great school owes no small part of its present international reputation and prestige to what Dr. Chandler built into it.

Eleven of the twenty-five years during which Professor Chandler taught at the College of Physicians and Surgeons were coincident with his presidency of the New York City Board of Health, and his students had the unusual opportunity of listening to lectures upon modern sanitation and hygiene by the man who was himself making the history of these subjects for their city.

V. AS AUTHOR AND EDITOR

As an author, Professor Chandler has a long list of publications to his credit, embodying the results of research and invention, official reports, public addresses, sanitary regulations, analyses of waters, minerals, milk, et cetera, a survey of which shows at once the wide range of his interests and the versatility of his genius. These will be found in the bibliography with which this biography concludes.

As an editor, he supplied for several years an American Supplement to the monthly edition of the *Chemical News* (of England); and, in July, 1870, in association with his brother, Professor William H. Chandler, of Lehigh University, founded the *American Chemist*, and continued its publication until April, 1877, when it was given up, to make way for the *Journal of the American Chemical Society*. Another editorial task, and one which proved most laborious, was that of chemical editor for "Johnson's Encyclopedia."

VI. AS BUILDER OF AMERICAN CHEMICAL ORGANIZATIONS

1. The American Chemical Society

The founding of our great national organization of chemists, the American Chemical Society, came about in the following way. (*American Chemist*, 5, 35-114 (Aug.-Sept.) 195-209

(Dec.) (1874); 6, 401-406 (May) (1876); "Proc. Am. Chem. Soc.," 1, 3-18 (1876); and "The Organization of the American Chemical Society," by William H. Nichols, in "A Half-century of Chemistry in America, 1876-1926. An historical review commemorating the fiftieth anniversary of the American Chemical Society," edited by Charles H. Browne, Philadelphia, Sept. 6-11, 1926, Chapter II, pages 11-16).

At a meeting of the Chemical Section of the New York Lyceum of Natural History, May 11, 1874, President J. S. Newberry in the chair, the subject of a Chemical Centennial was discussed and, on motion of Dr. Henry Carrington Bolton, the following resolutions were adopted:

"Whereas the discovery of oxygen by Joseph Priestley on August 1, 1774, was a momentous and significant event in the history of chemistry, being the immediate forerunner of Lavoisier's generalizations on which are based the principles of modern chemical science; and

"Whereas a public recognition of the one hundredth anniversary of this brilliant discovery is both proper and eminently desirable; and

"Whereas a social reunion of American chemists for mutual exchange of ideas and observations would promote good fellowship in the brotherhood of chemists: therefore

"Resolved that a committee of five be appointed by the chair, whose duty it shall be to correspond with the chemists of the country with a view to securing the observance of a centennial anniversary of chemistry during the year 1874."

President Newberry subsequently appointed the following committee: Dr. H. C. Bolton, chairman; Professor C. F. Chandler, Professor Henry Wurtz, Professor A. R. Leeds and Professor C. A. Seeley, by whom the Centennial was organized and the call issued. Acting upon the suggestion of Professor Rachel L. Bodley, of the Woman's Medical College of Pennsylvania, the meeting place selected was at the grave of Priestley, Northumberland, Pennsylvania, and there, on July 31 and August 1, 1874, the chemists of the country assembled and organized by the election of Professor C. F. Chandler as President of the Centennial.

This is not the place to describe in detail this memorable gathering, which marked the transition from the earlier epoch of American chemistry to the present one which is characterized by the cooperation of American chemists in great national societies in place of the former local or regional organizations.

After this Centennial Meeting of 1874, the project of establishing a chemical society was often broached by many of the New York chemists, and it was finally determined to call a meeting for the purpose. As was to be expected, Chandler immediately became the recognized leader of the movement and, under date of January 22, 1876, a self-constituted committee, of which he was the guiding genius and spokesman, sent out a circular letter to all chemists in New York and vicinity inviting cooperation in the formation of a local chemical society, and submitting a tentative draft of a constitution and by-laws for an American Chemical Society. Replies were received from forty or more assuring the Committee of their interest and support. Encouraged by this result, the Committee decided to attempt the organization of a national, instead of a purely local society, and to this end sent out a second circular letter to a much larger and more widely distributed list of chemists.

The response to this circular was so gratifying that the Committee issued a call on March 27, 1876, for an organization meeting, which was held on Thursday evening, April 6, 1876, in the lecture room of the University Building, New York College of Pharmacy, at the corner of Waverly Place and University Place, New York, with thirty-five chemists in attendance and with Professor Chandler in the chair, and the American Chemical Society was born.

Naturally, there was some opposition to the launching of this new undertaking. It came partly from those interested in the Chemistry Section of the New York Academy of Sciences, formerly the New York Lyceum of Natural History, and partly from those chemists who were ardent supporters of the Chemistry Section (Section C) of the American Association for the Advancement of Science. Then, too, there was the usual quota of timid and conservative souls who feared for the success of the venture and dreaded to take the plunge. These

various opponents questioned the need of such a society, either locally or nationally, because of the existence of the chemistry sections mentioned, doubted the ability of its organizers to enroll many members, to secure a sufficient number of meritorious papers for the meetings or funds for publication and general expenses, and were inclined to view the entire movement as inopportune and unnecessary.

Fortunately these conservatives were decidedly in the minority. The progressives rallied overwhelmingly to Chandler's support, and when the motion to proceed with the organization was put there were but three dissenting votes. A Constitution and By-laws were adopted and the organization completed by the election of officers and various committees. Dr. John W. Draper was elected President, and Chandler one of the vice presidents.

Among those most active in the support of this venture was another of the great builders of American chemical industry, William H. Nichols, who at that time uttered the following prophecy: "We do not come here expecting to find a society ready formed, with a library and a fine building; those will come in time. There is enough enthusiasm among the chemists to give us them by and by." Two of the men largely responsible for the fulfillment of that prophecy were Chandler and Nichols, the former of whom was President of the Society in 1881 and 1889, and the latter in 1918 and 1919.

Chandler was thus the father of the American Chemical Society. The handful of members enrolled in its first year has grown to over seventeen thousand, and this vast membership, far exceeding that of any other chemical organization in the world, is drawn not only from our own land, but from all quarters of the globe. The income of \$1780 of 1876 has swelled to an annual budget of approximately \$500,000. There are now more than twice as many Local Sections as there were chemists at the organization meeting. To take the place of the *American Chemist*, the Society publishes three great journals, *Chemical Abstracts*, *Industrial and Engineering Chemistry* and the *Journal of the American Chemical Society*, which circulate among all the civilized nations of the world.

2. The Chemists' Club of New York

Although Professor Chandler was not the originator of the movement which led finally to the establishment of the Chemists' Club of New York, he had always spoken in favor of such an affiliation of our New York chemists and was the unanimous choice of the membership for the Club's first president. Without his wise and experienced guidance, his unquenchable optimism and his financial assistance, the Club would have had a hard struggle for existence. Through fair weather and foul, he was its outspoken and undismayed champion.

VII. AS INDUSTRIAL CHEMIST

Chandler was one of America's first great industrial chemists, and aided and enriched almost every chemical industry which this country supported during the two decades following 1866.

His services were always in demand by those great corporations whose manufacturing processes were based upon chemistry, for he had not only a thorough knowledge of that science, but also an alert mind stored with original and valuable ideas. He was generally recognized as the highest authority in his day in this country in the field of industrial chemistry. While other able and eminent chemists had their single specialties, Chandler was called a specialist in all branches, and was particularly expert in sugar refining, petroleum refining, the manufacture of illuminating gas, photo-mechanical processes, and calico printing. He served as chemist for the New York Gas Company, and for the New York Steam Sugar Refining Company, as consultant for the Standard Oil Company and for various other great business interests.

He had no rival to the title of dean of the industrial chemists of the United States. Even at the ripe age of 83, he labored daily in the offices of the Chemical Foundation, Inc., helping to prepare our country for the international chemical competition he felt certain that we would have to face.

One of his first and far-reaching contributions to the progress of chemical industry was the invention of the system of Assay Weights, as outlined in his article entitled "A new system of

assay weights," which appeared in the American Supplement to Chemical News of August, 1869. Although this system has been for years the one used by all assayers and metallurgists throughout the civilized world, it is safe to say that but few of them are aware of the fact that it was Chandler who introduced it, with the aid of Becker & Son who manufactured the weights according to his specifications.

His observations and belief in the possibilities of the Castner Process for the production of caustic soda by electrolyzing sodium chloride in a "tipping cell" with a flowing mercury cathode, were communicated to the Mathieson interests and they later, acting upon his advice, purchased the American patent rights and established in this country a great electrochemical industry at Niagara Falls, New York.

Water was one of the subjects to which he early turned his attention, and complete analyses were made of nearly all the famous Saratoga Springs waters. His first important commission was the investigation of boiler incrustations and feed waters for the New York Central Railroad Company, which investigation was begun in 1863 and completed in 1864 at the School of Mines.

His first publication after coming to New York was "Report on water for locomotives and boiler incrustations, made to the President and Directors of the New York Central Railroad Company, including analyses of waters between Albany and Niagara Falls, and analyses of incrustations." It was an octavo pamphlet of thirty-five pages, and appeared in 1865. It proved his interest in the practical problems of the community, and was his first contribution to the important field of water supply, in which he soon became one of the country's recognized leaders.

Other papers which indicate his activity in this same field were the following:

Sanitary qualities of the water supplies of New York and Brooklyn. Report to the Metropolitan Board of Health, 8 vo., 9 pp., New York, 1868.

Analysis of the Ballston Artesian Spring (with E. Root); *American Supplement to the Chemical News*, 1869, 54 (July).

Analyses of six new mineral springs at Saratoga; *American Supplement to Chemical News*, 1869, 194 (Sept.).

Analysis of the Saratoga Seltzer Spring (with Paul Schweitzer); *American Supplement to Chemical News*, 1869, 395 (Dec.).

Report on the water supply of New York and Brooklyn, made to the Metropolitan Board of Health; 8 vo., 9 pp., New York, 1870.

Analyses of the Chittenango Sulphur Springs, Madison County, New York; *American Supplement to the Chemical News*, 1870, 221 (April).

Saltiness of the waters around the island of New York; *American Supplement to the Chemical News*, 1870, 225 (April).

Analysis of the Geyser Spring of Saratoga (with F. A. Cairns); *American Supplement to the Chemical News*, 1870, 373 (June).

Lecture on water; delivered before the American Institute; 8 vo., 49 pp., Albany, 1871.

Lecture on water (revised and elaborated); *American Chemist*: 1. General, 1871, 161 (Nov.); 2. Mineral Waters, 1871, 201 (Dec.); 3. Water for manufacturing and domestic purposes, 1872, 259 (Jan.), 281 (Feb.); 4. The Croton, 1872, 321 (Mar.).

Analysis of the Florida Sulphur Spring; *American Chemist*, 1871, 300 (Feb.).

Analyses of Staten Island waters (with F. A. Cairns); *American Chemist*, 1871, 347 (Mar.).

Report on the water of the Hudson River; made to the Water Commissioners of the City of Albany. A special discussion of the destruction of the sewage contamination of large rivers, caused by the dissolved oxygen; 8 vo., 25 pp., Albany, 1872.

Analysis of the Empire Spring at Saratoga (with F. A. Cairns); *American Chemist*, 1872, 93 (Sept.).

Analysis of the Glacier Spouting Spring at Saratoga (with F. A. Cairns); *American Chemist*, 1872, 165 (Nov.).

It may be of interest also to note in passing, a fact which is probably unfamiliar to many, namely that it was due to Chandler's advice and urging that the firm of Eimer and Amend started their laboratory supply business in 1874. The founders of this firm were German apothecaries who were dissatisfied with the standards of purity of the American pharmaceutical products of that day and therefore began the importation of alkaloids, etc. Chandler, who was personally acquainted with them through his connection with the College of Pharmacy, called at their office and urged them to extend this branch of their business and to include also the importation and manufacture of chemical apparatus. How great an assistance it has been to the cause of American chemistry to have had at hand the stores of fine chemicals and apparatus provided by this firm, it is difficult to overestimate.

So much of his time was occupied in great patent suits that but little was left for investigation. I well recall his advice, when I asked him to give me some guiding principles in serving as chemical expert in such litigation. His answer was brief and to the point: "Know your lesson. Tell the truth. Don't be afraid to say 'I don't know.'"

The most famous suit in which he participated was that brought in the interest of the widow and assignees of the Rev. Hannibal Goodwin, an Episcopalian minister of Newark, N. J., and an amateur photographer, who was the first successful inventor of a flexible nitrocellulose film for the kodak camera and for moving picture negatives and positives. Goodwin's opponents kept him in the Patent Office ten years before he could get his patent, when he died. Thirteen years of most strenuous litigation were required to win against the infringer. Rather than have a Court investigator examine the books to determine the proper award, the defendant settled with the plaintiffs by the payment of \$5,000,000, which is believed to be the largest amount ever secured in a patent suit up to that time.

Other noteworthy suits were the first synthetic coal tar color cases, of which there were five concerned with Artificial Alizarin, and others later which had to do with the Roussin patents, owned by Poirier & Company, of Paris, for the Azo

Dyes obtained by diazotizing sulfanilic or naphthionic acid and coupling with suitable amines or phenols; but to review even briefly the many notable suits in which he has been engaged would require more space than is available for this biographical notice, for they included subjects in almost every branch of chemical industry.

Sugar

Dr. Chandler's connection with the sugar industry began in an interesting way.

The Kappa Alpha Fraternity, of which he was a member, held a banquet in New York which he felt that he really should attend, although the tickets cost \$6.00, and his annual income was very meager. Mrs. Chandler also urged him to go, for she knew that the recreation and change would rest him. So, to the banquet he went, and enjoyed himself so thoroughly that he spent \$18.00 in treating his comrades to proper liquid refreshment. The cost of supplying this pre-Volstead enjoyment, plus the cost of the ticket, made the expense of the evening \$24.00, as he explained with rueful countenance to his wife upon his return home. She cheered him, however, by suggesting that after all his presence at so important a gathering might later prove to have been worth while.

And so it proved. For one of the guests at the dinner was Mr. Peters of the Booth & Edgar sugar refinery, at King and West Streets, who was so much pleased with his acquaintance with Chandler on that occasion that he later approached him with an invitation to do their chemical work for them. They recognized the need of proper scientific control of the operations of their plant, but had not succeeded in finding a satisfactory man for the job. Mr. Peters expressed the belief that Chandler could do all the work they required if he could give them about two hours a day, for which they would pay him \$1500 per annum.

As this would materially increase his income, Chandler accepted the offer, and on May 1, 1888, began work for them on this basis, his hours being from six to eight A. M., and

his work consisting of analyses of sugars, syrups, boneblack, et cetera, polarimetric determinations, and the study of various chemical problems, both in the laboratory and in the plant.

VIII. AS PUBLIC SERVANT

1. In the Service of New York City

Board of Health.—Unquestionably Dr. Chandler's greatest services in the utilization of science for the benefit of mankind were in safeguarding the public health and saving human lives through improved sanitary measures.

In 1866, he was invited by Dr. Harris of the Metropolitan Board of Health to make some scientific studies of various sanitary questions affecting the health of the community. Although there was to be no pay for the service, Dr. Chandler undertook it unhesitatingly, and the commissioners were so well satisfied and so convinced of the importance of the work that at the end of the year they created the position of Chemist of the Board of Health for him, a position which he held until 1873, when he was appointed President of the Board by Mayor Havemeyer, and in 1877 re-appointed by Mayor Ely, his second term expiring May 1, 1883.

Devoting himself assiduously to all branches of hygiene and sanitary science, he studied with the utmost care and thoroughness all factors bearing upon the health of a great city.

Among the many important matters included within the wide sweep of his investigations were the gas nuisance, dangerous kerosene, the city water and milk supplies, adulterated liquors, poisonous cosmetics, offensive trades, the removal of the stalls around Washington, Fulton and Centre Markets, the abatement of the sludge acid nuisance, the regulation of slaughter houses, cattle driving, the construction of tenement houses, the introduction of a proper system of plumbing and house drainage, the establishment of the summer corps of visiting physicians, the permanent system of gratuitous vaccination, and the proper care of contagious diseases in special hospitals. It is desirable to consider a few of these reforms in somewhat fuller detail, so that we may see more clearly just what they meant to the city.

The *water supply* was found to be entirely satisfactory, the adulteration of *liquors* less than was expected, but *cosmetics* were causing some trouble because of the frequent presence therein of poisonous lead compounds, the *gas* industry was creating and maintaining nuisances, and dangerous burning oil was taking its toll of human lives through numerous lamp explosions.

One of the first and most successful investigations was that of the cause of kerosene accidents.

The subject of artificial illumination had always attracted him. New Bedford, his home town, was the whaling headquarters of the United States. Whale oil, sperm oil and spermaceti were everywhere in evidence. When the boys burned any "midnight oil" in the study of their lessons, it was whale oil which supplied the illumination, and when they were punished by being taken upon a whaling expedition to the back shed, it was whalebone switches that extracted the blubber. On account of the high price of sperm oil, and the inferior quality of whale oil (from the "right" whale) and its property of gumming up the lamps, "camphene" (rectified spirits of turpentine) was introduced at comparatively low cost as a substitute. This camphene could not be burned in lamps without chimneys on account of its smoking. It was therefore mixed with alcohol, so that it could be used in open wick lamps without any chimney, and this mixture was sold under the name of "Burning Fluid." Both this and camphene were exceedingly dangerous, because of the inflammable vapors they gave off even at ordinary temperatures, and many fatal accidents resulted from their use.

This was the situation when he left New Bedford in 1854 for Germany. In the fall of 1855 he moved from Göttingen to Berlin and arranged to board with a private family there. The first evening when he lighted his lamp, he observed at once that it was filled with a kind of oil with which he was unfamiliar and which he was told was called "Photogen." The following day, when he went to the University, he learned from Professor Magnus, the lecturer on Industrial Chemistry, that it was made by the distillation of "Boghead Coal," or "Torbane Hill

mineral" from Scotland. He became very much interested in this new oil and gathered all the information he could about it. On returning to New Bedford in the fall of 1856, he told his friends all about this new oil, especially his uncle who owned a large refinery and candle works, but they all derided the idea of its being good for anything, or of its ever supplanting whale or sperm oils for either illuminating or lubricating purposes.

Unconvinced, Dr. Chandler then turned to the only scientific journal with which he was familiar in this country, the *Scientific American*, and wrote its editors, Munn & Company, about the new oil, offering to supply gratuitously an article on the subject. He received a reply that they "did not care for the article," as they "did not think this kind of oil would ever interest the American public." But within two years there were coal oil factories from Portland, Maine, to Wilmington, Delaware, including one at New Bedford, making oil from various shales (usually Boghead coal), and one of these plants called its product "kerosene," a name which has clung to burning oils ever since, whether obtained from coal or from petroleum.

The coal oil industry soon gave place to the distillation products obtained from petroleum. Accidents due to the explosion of kerosene were then of frequent occurrence, but inasmuch as similar accidents had occurred with the various burning fluids mentioned above, the layman supposed that such accidents were either unavoidable and due to the peculiar properties of such substances, or else were caused solely by carelessness.

Dr. Chandler suggested to the Board of Health that the cause of these accidents be examined into more thoroughly, and they requested him to undertake the work.

On January 11, 1869, he submitted his report, and it was subsequently published in the *American Gas Light Journal*, of February 2, 1869, under the caption, "Dangerous Kerosene." It pointed out the fact that kerosene was only dangerous when it contained the cheaper and more volatile naphtha fractions. There were profiteers in those days also, and the greed of certain grocers had led them to mix safe refined kerosene with the

much more dangerous but cheaper naphtha or benzine. The inevitable result of this criminal practice was an appalling loss of life wherever this highly inflammable mixture was used for illuminating purposes, and scarcely a day passed in New York City without one or more accidents from lamp explosions, some of which proved fatal.

Chandler examined seventy-eight samples of oil purchased from retail dealers in the city, and not a single one was safe, while many were mainly naphtha sold under the ironical title of "safety oil."

This report created tremendous excitement, not only in New York but also throughout the entire country and even in Europe. Boards of Health in various American cities had it reprinted and distributed it broadcast; and it was quoted in many foreign countries.

Its direct effect was the introduction of a safe burning oil for the world. In the three years following, this led to a reduction in the death rate from lamp explosions in New York City from fifty-two per million to fifteen per million, with further reduction later. To the petroleum industry, it brought rapid and profitable growth in the sale of kerosene, a substance whose use previous to this report had been looked upon with more or less disfavor by the general public. To Chandler, it brought instant recognition and widespread renown. In 1872, he was invited to appear before the British House of Lords in regard to his investigations in this field.

Some years afterward, he was sent to Europe by the Standard Oil Company, visiting Germany on one occasion and England on another, the object of the visits being to induce the government authorities in these countries not to enact laws which would have been inimical to the interests of American producers and would have reduced materially the volume of American oil exported. In these missions he was entirely successful.

Markets.—One of the first reforms undertaken by Dr. Chandler in 1873, shortly after his election as President of the Board, was the destruction of the two-story structures which covered half the roadway of the public streets adjacent to Washington

Market, surrounding the block and interfering with the traffic in these thoroughfares. They were in a filthy condition, and great quantities of refuse had accumulated beneath them, giving rise to offensive odors and tending to taint all the food exposed for sale in the market. Many attempts to remove them had been made, but without success. After exhausting peaceful methods, the Board decided to resort to force, and Dr. Chandler was given authority to put Washington Market in order. The Board of Encumbrances refused to obey his orders to remove the stalls. The police likewise refused. The marketmen inquired whether a purse of \$50,000 would "square the matter." Threats of personal violence were also not lacking. Dr. Chandler was haled into court to show cause why a preliminary injunction should not be issued restraining him from tearing down the structure. The judge declined to issue such a preliminary injunction, but stated that at 10 o'clock next day he would be required to show cause why a permanent injunction should not be issued. On leaving the court room his counsel said, "This means that what you do between now and 10 o'clock tomorrow morning will be legal. What you may do after that time depends upon what the court may say."

With his indomitable energy and resourcefulness, Chandler went to work at once. He found an old house-wrecker who agreed to demolish and remove the structure for \$2500. But, under the law, no contract for city work for \$1000 or over could be let without advertising for bids. So he offered the wrecker \$999 to demolish the buildings, and then offered his foreman \$999 on a separate contract to remove them. He went before the Board of Police Commissioners demanding police protection for the wreckers. The commissioners laughed in his face, although the President of the Board of Police was himself a member of the Health Board. Chandler thereupon told them that he would at once prepare and file an affidavit to the effect that he had made formal demand for police protection for his men and had been refused. This affidavit would be placed where it would settle definitely the responsibility for any rioting that might occur. He went then to his own office in the same building, and in a few minutes received word that

the police protection would be forthcoming. Sixty of his own sanitary police were sent to remove the contents of the stalls to the inner portion of the market and three hundred of the regular police force formed a cordon around the structure. The attack began at eight o'clock in the evening, and by ten o'clock the next morning all of the ramshackle structures had disappeared, the débris had been carted away, and the pavement, which had not seen the light of day for forty years, had been washed clean by the Street Cleaning Department. When the case was called in court a laugh went around, for there was nothing for the court to do, as the marketmen decided not to submit their petition. A suit was subsequently brought against the city for about \$60,000, but the city won it. Similar treatment was accorded the stalls surrounding the other city markets.

The whole undertaking was not at all an unjustifiable high-handed proceeding, but a wise protection of the health of the great metropolis, for there was at the time an epidemic of cholera in Memphis, whose northward spread was causing anxiety in many of our cities, and unsanitary markets were recognized as among the best possible breeding places for such scourges.

Smallpox.—During the winter of 1874-75, three thousand cases of smallpox were reported in New York City, with twelve hundred deaths. It had been endemic since 1800, but was then epidemic. In those days, the patients were sent to Blackwell's Island and were nursed by the "seven day drunks" from the workhouse north of the hospital. The principal medicine administered was whiskey, much of which was drunk by the nurses. The tales of the cruelty and indifference with which the sufferers were treated reached the poorer quarters of the city and the families of the patients refused to report the cases. As a consequence, the first intimation of the presence of smallpox often came only when an entire tenement had been infected. The patients then were under the control of the Department of Charities.

It was one afternoon at about four o'clock that Dr. Chandler received word of the passage of the bill giving the Board of Health entire control of contagious diseases. He had himself

vaccinated immediately, and visited the hospital for smallpox, so that he might see for himself the true state of affairs, viewing for the first time a case of the disease, of which there were three hundred there at the time.

The first thing the Board did was to change the name of the hospital to Riverside Hospital, in place of Smallpox Hospital. Then Drs. Chandler and Smith went to Archbishop McCloskey and explained the situation to him, with the result that he furnished fourteen Sisters of Charity to replace the "seven day drunks" as nurses, and to be under the direction of a competent resident physician. Instead of the ambulance, or Black Maria, a fine coupe, drawn by a span of handsome gray horses, was provided. There was no label on the door publishing to all the world the nature of its business. On the box sat a uniformed coachman, and at his side a groom, and the outfit might have passed down Fifth Avenue any sunny afternoon without causing anything but envious comment. When this equipage appeared in the lower East Side, it created a sensation, and there was little objection to being driven away in style to a hospital where the poor knew that they were to have devoted Sisters of Charity as nurses. The effect was immediate and far-reaching. Smallpox cases were no longer concealed, and the epidemic was soon stamped out, with the assistance of the corps of vaccinators employed and the systematic house-to-house vaccination adopted.

The question of universal vaccination was approached diplomatically, in order not to stir up too much controversy. Instead of seeking a compulsory general vaccination law, they addressed the Board of Education and induced them to pass a regulation refusing admission to the schools to all children who had not been vaccinated. As the law required all children within certain ages to go to school, the desired result was accomplished, and this reform has been in force ever since.

Hospitals for contagious diseases were erected on North Brother's Island to take the place of the one on Blackwell's Island, although this reform was not completed until after the termination of Dr. Chandler's service on the Board.

At first, the vaccine was prepared from human beings. There was considerable opposition to this, and an Anti-Vaccination Society was organized, but this difficulty was readily met by the Board's hiring a farm in New Jersey and preparing all virus needed by using calves.

Milk Supply.—Numerous analyses of the milk sold in the City of New York, clearly established the fact that this important food was so heavily adulterated that for every three quarts of milk supplied, one quart was water; in addition to which a considerable percentage of the cream had been removed. It was also found that most of the condensed milk companies skimmed the milk before concentrating it. The total frauds thus perpetrated by the milk-men were estimated as amounting to \$10,000 a day. The Board of Health had not attempted to grapple with this villainy until Dr. Chandler became its President. He then promptly inaugurated a vigorous warfare against the dishonest dealers on the grounds that as milk was the chief food of the 130,000 children under five years of age then living in New York, it was a most important article for sanitary supervision.

Impure or watered milk was dumped into the gutter by the thousands of gallons, and the offending milk-men were arraigned and fined or imprisoned. The milk dealers organized an association to resist this attack, engaging legal and chemical experts and fought both the law and the chemical methods employed, but the Board of Health won its suits and the best chemists in the country approved the methods employed. Fifty thousand dollars were paid into the city treasury as fines by guilty milk dealers, and a number of them enjoyed the hospitality of the city jail from ten to ninety days apiece.

Air Pollution.—Not content with having safe-guarded the food and water supply, the improvement of the conditions surrounding its great markets, and rigid inspection of its drinking water, Dr. Chandler next turned his attention to the purification of the air then being breathed by New Yorkers.

For many years most intolerable odors prevailed over great areas of the city, now on the east side, and now on the west side. These odors were at first attributed to the sewers, but it

was later ascertained that the gas companies were the guilty parties.

It is to their credit that when this matter was brought to their attention they, with one exception, modified their processes so as to suppress the odor, the immediate cause of which was found in the foul lime which was daily removed from the gas purifiers.

The one company which refused to pay any attention to the suggestion of the Board, claimed that it was not producing any disagreeable odor, that it was good for whooping-cough, that what odors were produced were inevitable, and that any change in their process would leave their gas so impure that it would not be safe to burn it in dwellings. A long trial before a referee ensued, in which the intransigent company was decisively beaten. The necessary changes were made and the gas nuisance ceased.

Other sources of pollution of the atmosphere were discovered in bone-boiling and in fat and refuse-rendering, and the stench from these plants often made the air of the city in certain sections well nigh intolerable.

The first attack by the Board of Health was made upon the New York Rendering Company, which had a contract with the city for the removal beyond the city limits of dead animals and offal. Instead of complying with the terms of their contract, the offensive material was rendered at the foot of West Thirty-eighth Street. The company was directed to stop rendering at once, and to live up to their contract by carrying the material away from the city.

At first they claimed it was impossible, but when policemen were placed on the dock, with orders to arrest the workmen if they attempted to continue the work, the company transferred the material to a vessel, carried it down the bay, and threw it overboard.

As most of it floated ashore it became the cause of equal offense in other localities. The company insisting that it must either "render or dump," the Board of Health declared the contract violated and made arrangements with other parties to transfer all the material to Barren Island, and these arrangements have been in force ever since, with entire satisfaction.

The Rendering Company, still retaining possession of their dock at the foot of West Thirty-eighth Street, placed every obstacle in the way of the new contractors and it became necessary to seize all their movable property, put it upon their two hulks, which had been tied up to the pier, and tow them over to the New Jersey shore. This resulted in a suit against the city for over \$100,000, which was defended by the Board with complete success. Some of the details of this long and bitter controversy will be found in the May 17, 1883 issue of "The Sanitary Engineer."

The next attack was upon the vessel *Algonquin*, which lay at the foot of Thirty-ninth Street, and on which offal from slaughtering houses was rendered. Orders issued directing the owners to discontinue the business were resisted and the industry was moved out into the stream, so as to carry on the work out of the reach of the inspectors.

The first encounter was a night attack upon the trucks engaged in carrying the offal to the end of the pier for delivery to the vessel. As this failed to put a stop to the business, a steam tug was hired and the *Algonquin* towed to Barren Island and tied up there. This put a final quietus on the business.

A gut cleaning establishment at the foot of West Thirty-ninth Street next received attention and the Board finally sent a gang of laborers which tore down the building and burned up the material, the owner being compelled to pay the cost which was \$125. Bone boiling, fat melting and lard rendering were sharply followed up, until every establishment engaged in the business was compelled to introduce improved processes which could be carried on without polluting the atmosphere.

Slaughter Houses.—Slaughter houses were scattered all over the city. On Manhattan Island alone there were fifty-two, and cattle were driven to them through the streets. The Board of Health, at the suggestion of President Chandler, encouraged the erection of a few large abattoirs near the East River, on First Avenue near Forty-second Street, where they remained for many years. The driving of cattle through the city streets was prohibited and it was required that they be transported by water instead.

The streets adjoining the slaughter houses actually ran with blood, which flowed from the gutters into the sewers and thence to the river. When the Board of Health forbade this, it was bitterly fought until Dr. Chandler showed the offenders how by the installation of proper equipment for drying this blood they could market it as a valuable fertilizer and make money out of it.

Night Soil.—Another source of air pollution was the removal of the night-soil from the twenty-five thousand privy vaults of the city's tenement houses. The contractors who undertook this work carried it out in such a slovenly manner as to constitute a serious nuisance. The Board of Health therefore declared the contract violated, and negotiated a new one with other parties at considerable saving financially and with the result that the nuisance was abated. The original contractors thereupon sued the city for \$300,000 and were defeated.

Petroleum Nuisances.—At Sixtieth Street and the Hudson River there was a petroleum refinery which spread offensive odors over all the neighborhood. Dr. Chandler sought in vain to get this company to alter its methods of refining so as to prevent this pollution of the air. As it continued stubbornly to refuse to do anything to reduce or stop this nuisance, the refinery was driven from the city.

Another source of contamination of the city's air was the fertilizer industry at Hunter's Point and Greenpoint, where the sludge acid from the petroleum refineries was used in the manufacture of superphosphates.

As the Legislature was averse to enacting laws for the suppression of nuisances in one place which had their origin in another place, the Board secured the necessary authority under a special act designed for another purpose, and proceeded to gather evidence fixing the exact times at which these various factories emitted foul odors. The officers of the Board of Health were then suddenly indicted on the ground that they had failed to suppress foul odors arising within the city limits, but this indictment was promptly quashed and Dr. Chandler and his fellow officers, with the new powers of their Board

acknowledged, turned on their late accusers and forced them to abate the nuisance.

The Summer Corps.—The vicissitudes of our climate are such in New York City that during part of the mid-summer months of July and August, we frequently suffer from very depressing and debilitating weather conditions with temperature and humidity excessively high. Such weather falls with particular severity upon the little children, and consequently results in adding several hundred to the ordinary death rate. It has happened for example, that in a single week the death rate has risen from five hundred to fifteen hundred, nearly a thousand children falling victims to the diarrhoeal diseases induced. To reduce as far as possible this excessive mortality, Dr. Chandler organized a Summer Corps of fifty physicians, one for each of the fifty districts into which the tenement house regions were then divided; it being the duty of each of these physicians to visit every room in this district, treating all sick children, furnishing the medicine required, giving advice to the mothers, placing in their hands instructions for the care of infants, printed in English and other languages, and distributing tickets for excursions on the barges of St. John's Guild.

As an illustration of the work accomplished, it is stated that the physicians of the Summer Corps in one month made 130,000 visits at a cost to the city of \$5000. The beneficent effects of such work enabled the Board to secure each year the appropriation necessary to continue it.

Tenement House Reforms.—Statistics having shown that two-thirds of the deaths in the city occur in that half of the population in tenement houses, efforts were made to improve the conditions of these structures by careful inspection and by suitable remodelling where possible. It was soon found, however, that this reconstruction was a difficult matter with the average tenement house building and that there should be radical changes made in the plans for such buildings, in order that they might provide adequate light and ventilation.

With the aid of the *Sanitary Engineer* and a number of public spirited citizens, the Sanitary Reform Association was organized which, working in conjunction with the health author-

ities, secured the enactment of such amendments to the Tenement House Act as gave the Board of Health complete control of all tenement houses thereafter to be erected. In the first three years after that law went into effect, accommodations were provided in new structures for approximately one hundred thousand persons, and in every case ample light and ventilation were supplied.

Plumbing and Drainage.—It was early recognized by the Board of Health that many serious defects existed in the plumbing and drainage of dwelling houses, by which exhalations from the sewers, often carrying germs of disease, found access to living and sleeping rooms. This was due not only to defective execution of the work, but also to the fact that in principle the system employed was unsafe.

Dr. Chandler therefore undertook experiments designed to provide a satisfactory system and one which would remedy these evils. It was soon found, however, as the result of these investigations, that no house was safe from sewer gas unless a soil pipe of undiminished calibre was carried through the roof, a trap placed between the house and the sewer, suitable traps placed under every fixture, every trap backaired to prevent syphoning, and all overflows, refrigerator and safes wastes disconnected from the sewer.

These improvements were so obvious and commended themselves so promptly to every intelligent citizen, that the Sanitary Reform Association secured the enactment of a law for the registration of all plumbers and for placing the control of plumbing and drainage for all new buildings in the hands of the Board of Health. This law went into effect June 4, 1881, and the rules adopted by the Board, after consultation with leading plumbers, architects and sanitary engineers, have proved so satisfactory that they have been adopted by many other large cities.

Effect upon the Public Health.—In 1866, out of every one hundred deaths in the city, fifty-three were of children under five years of age. The efforts at sanitary improvement so actively conducted under Dr. Chandler's leadership, together with the increased intelligence secured by the efforts of the sanitary

authorities, the sanitary organizations and the press succeeded in reducing this ratio year by year until at the conclusion of Dr. Chandler's services, the fifty-three deaths per one hundred had been brought down to forty-six.

This meant on the basis of the city's population at that time an annual saving of the lives of five thousand children, without considering the reduction in the death rate of persons over five years of age. If the latter were also included, it is fair to estimate that at least eight thousand lives were saved to the city every year as a result of these sanitary reforms. As for each death there are twenty-eight cases of severe sickness, the total amount of good accomplished becomes enormous.

Largely due to Dr. Chandler's activity, a State Board of Health was established of which he was made a member and chairman of its Sanitary Committee. He was always active in endeavoring to secure suitable legislation prohibiting the adulteration of foods and drugs throughout the state, and when such laws were finally placed upon the statute books, their enforcement as well as the regulation governing the sale of kerosene, were entrusted to him. Public analysts were appointed, samples of food, drugs and oils collected, and the necessary machinery set in motion to secure compliance with these laws throughout the state. For three years he served as chairman of this Sanitary Committee.

Upon completion of his second term as President of the New York City Board of Health, May 1, 1883, Dr. Chandler was nominated by the Mayor for a third six-year term, but the Board of Aldermen refused to confirm the nomination, and he held over for about a year. The reason for this action by the Aldermen in failing to continue in office one who had done so much for their city merits more than passing mention, for it is a striking illustration of the eternal fight between politics and probity, and in this particular case, to the disgrace of our city be it said, politics won.

It had been customary to accumulate the stable manure from the eighty thousand horses in the city and store it in huge piles in open lots in various parts of the city, as at the foot of West Thirty-eighth Street, of East Forty-fifth Street and West Nine-

tieth Street, where it was soaked with the rain and then, under the rays of the sun, fermented and decomposed. To prevent these piles from overheating and taking fire, it was necessary to fork them over frequently, and the odors from these rotting piles polluted the atmosphere of the entire neighborhood. As the farmers took such fertilizer only in the spring, as a rule, these localities were sources of evil odors for a large part of each year. When the Board of Health stationed policemen at these points and prevented the contractors from storing manure there, the latter refused to take it from the stables, and the nuisance was transferred to thousands of localities throughout the city. After three years of litigation, it was forbidden to store such material within the city limits, and arrangements were then made by the Board to have it stored outside of the city, but in the meantime, the interested contractors had succeeded in having slipped through both branches of the Legislature a bill which would prevent the Board of Health from interfering with this objectionable business in any way. The lawyer for the contractors immediately paid a visit to Chandler, and spent an entire morning endeavoring to induce him to withdraw his opposition, assuring him that if he did so the contractors' aldermanic friends would see that his nomination was immediately confirmed. Chandler finally told him that he would rather be carried off to the cemetery than betray the people in such a manner, and the lawyer left, after telling Chandler that he was a fool to continue his opposition. As this bill had been slipped through in the closing days of the session of the Legislature, there was only that day left within which the Governor could veto it, so Chandler immediately wired him pointing out that the lives of over a million people would be jeopardized if the bill became law. Governor Cleveland promptly vetoed the bill, the Board of Aldermen refused to confirm Chandler's nomination, and New York City lost as efficient a public servant as it has ever been her good fortune to possess.

In addition to his service on its Board of Health, Dr. Chandler aided New York City in other directions also.

As President of the Street Cleaning Improvement Society, he performed an effective part in bringing about the reorganization of the Street Cleaning Department.

At the time of the opening of the New York Subway, he was called upon to analyze the air in the tubes, since many people claimed that it was injurious to health. His analyses showed conclusively that the air in the subway was just as pure as that above ground and the anxiety of the public about this matter was thus relieved.

He served also as one of the scientific directors of the New York Botanical Garden, and was for many years Chemist for the Croton Aqueduct Commission.

2. In the Service of New York State

His State also made use of his scientific training and administrative abilities in many ways.

For several years he was president of the State Charities Aid Association and took an active part in securing proper state care for the indigent insane.

3. In the Service of the Federal Government

Not only the city and state, but the nation as well called upon Dr. Chandler for public service. Secretary of the Navy, William C. Whitney, selected Chandler as a member of a commission to investigate the preservation of timber, and soon thereafter the President of the National Academy of Sciences, at the request of the Secretary of the Treasury, placed him on commissions to investigate the manufacture of glucose, the denaturing of alcohol, and the waterproofing of fractional currency and bank notes.

When Chester A. Arthur was President of the United States, he named Chandler a special commissioner to study the subject of American hog products and report thereon, and in 1884 appointed him a United States delegate to the Health Exposition and the International Medical Congress at Copenhagen.

President Cleveland, soon after his inauguration, sent for Professor Chandler, told him that the previous Congress had

appropriated \$400,000 to keep cholera out of the country, and that he wanted his advice because both the National Board of Health and the Marine Hospital Service had each urged that the money be turned over to them. Chandler promptly responded, "Don't give it to either!" as it would have been only a drop in the bucket for either one, while if he created a Cholera Commission of the State Quarantine officers of the larger Atlantic ports, it would enable them to add to the equipment they already possessed, in stations, steamboats, hospitals and staffs of officers, such other conveniences as might be necessary. The President accepted the suggestion at once.

Three successive Presidents, Cleveland, McKinley and Roosevelt, appointed him a member of the Visiting Committee of the National Observatory at Washington.

At the request of the Postmaster General, he investigated the postage stamps and refuted the stories of the newspapers that they contained poisonous constituents which made it dangerous to touch them with the tongue.

Twice, in 1880 and again in 1903, he was called upon by the Secretary of the Interior to examine the original Declaration of Independence, not from the standpoint of its justice or morality, but to advise how the faded signatures might be made more legible. Some years previously, a publisher had been allowed to take a wet paper press copy of these signatures, which removed most of the ink from the parchment. All that could be suggested was to go over the signatures with india ink, but this seemed too hazardous and nothing was done.

IX. OTHER HONORS

In addition to the honors already recorded, the University of Göttingen, upon the fiftieth anniversary of his Ph. D. degree, officially renewed this degree and sent him a parchment to that effect.

He received the M. D. degree from the University of New York in 1873, and the LL. D. from Union College the same year. In 1900, Oxford University conferred upon him its D. Sc. This was indeed a signal distinction, as the only previous recipient of this honorary degree from Oxford had been the Prince of Wales. Chandler was thus the first scientist upon whom it was conferred. In 1911, Columbia also awarded its LL. D.

He was a member of the National Academy of Sciences, of practically all the leading chemical societies both here and in Europe, and of many other scientific, learned and philanthropic organizations.

On April 30, 1910, a huge banquet was given in his honor by the chemists of America, and a bronze bust of life size was presented to Columbia University. It is an excellent likeness, and now is housed in Havemeyer Hall where all may see it and where it shows to all students what the great builder of their Chemistry Department looked like.

When the Society of Chemical Industry decided to recognize its large and influential American Section, and to draw closer the bonds uniting the two nations, it made inquiry as to whether there was any one American chemist who, by the common consent of his colleagues and in virtue of his achievements, might properly be regarded as first among our industrial chemists. There was no question that Chandler was, and had been for many years, the leading figure in this field, and he therefore received the unprecedented compliment of election to the presidency of this great international society, being its first American president, and thus joined the group of distinguished men who had held that high office previously.

He died August 25, 1925, in his ninetieth year, after a brief illness, mourned deeply by all who knew him or were familiar with his remarkable contributions to the happiness of mankind.

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OF

JOHN TROWBRIDGE
1843-1923

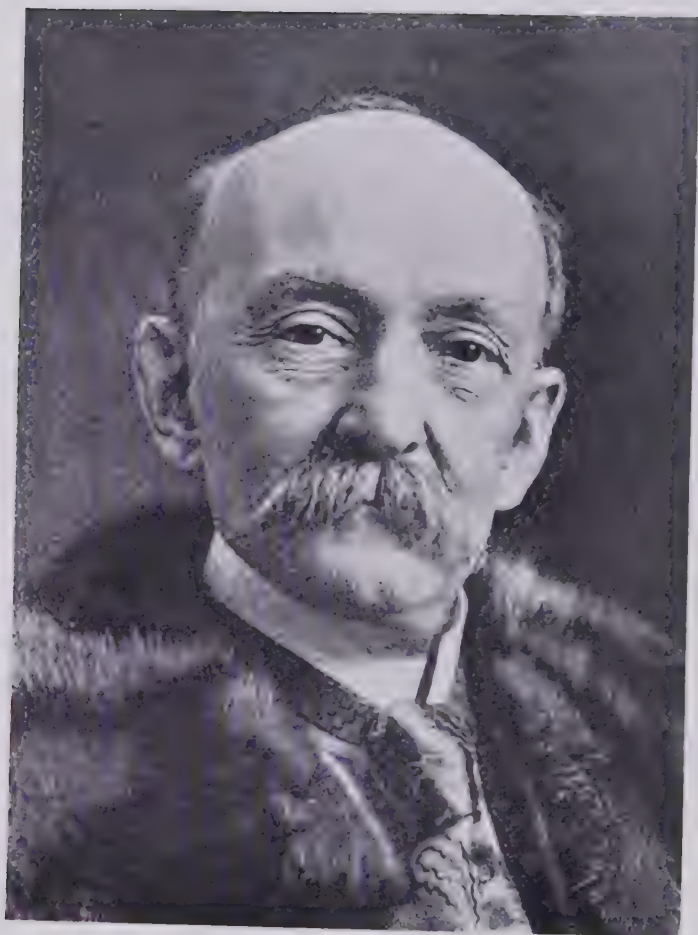
BY

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PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1930

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JOHN TROWBRIDGE

— —, 1843—February 18, 1923

BY EDWIN H. HALL

Our colleague John Trowbridge was the sixth in succession of his family to bear the name John. Such persistence in the use of a given first name indicates family consciousness, if not family pride, and is perhaps the closest approach in this country to the inheritance of a title. It is true that the first Trowbridge to come from England to America was named Thomas, but his father, "a wealthy merchant and prominent citizen of Taunton, Somersetshire," was called John.

It appears that Thomas Trowbridge (1) and his wife, with two sons, came to Dorchester in Massachusetts about 1636. They are mentioned, it is said, in the records of the town and of the church as "Mr. and Mrs.," "a distinction confined at that time to persons of established gentility." Thomas Trowbridge removed to the plantation of New Haven, Connecticut, probably about 1638. He went back to England after a few years, and it appears that he never returned to America, though he left three sons permanently in this country, with enough property to be the cause of a good deal of trouble between the family and the steward in charge of the estate. Two of the sons, Thomas and William, remained in New Haven and established the family name in Connecticut, but a third, James (2), returned to Massachusetts and lived in Newton, or New Town, which is now Cambridge. He was a soldier in King Philip's War, being a lieutenant of the Cambridge village company of foot soldiers, and he was also a deacon of the church, evidently a man of note in his community.

A son of this James, Thomas (3), removed to Connecticut but left in Massachusetts two sons, Edmund and John. The former remained in Cambridge, and Trowbridge Street in that city is named for him. He was a noted lawyer and judge, a Tory during the Revolutionary War. The brother John (4) settled in Framingham, where he was "house-wright," farmer and select-

man. His son John (5) also lived in Framingham, was farmer, tavern-keeper, selectman, a soldier in the French and Indian War, a lieutenant-colonel in the Revolutionary War. This John had a son John (6) who marched to Concord as a Minute-man, took part in the battle of Bunker Hill, was commissioned first lieutenant in 1779, and was town treasurer of Framingham for twenty-five years. His son John (7), born in Framingham in 1778, removed to Cambridge, engaged in trade and largely increased the property he had inherited from his father. The son of this John was given a middle name by way of innovation, being called John Howe (8). He entered Harvard College but did not complete the course. He graduated from the Harvard Medical School in 1835 but, having inherited a fortune, did not feel the need of practicing his profession. He was the father of our colleague, John (9), who was born in Boston in 1843.

This is an excellent New England family record and it accords well with the impression which Trowbridge made on everyone as of a man born to good conditions. The financial ease of his father did not, however, endure, and Trowbridge has told me that, while still a youth, he had to begin earning money. He had artistic talent and painted pictures which found purchasers. Possibly he began at this time the habit of literary composition which he continued for many years. The esthetic element was very evident in him; in addition to painting and semi-imaginative writing, he had music, the piano, as an accomplishment. It seems quite possible that, if he had been under no financial compulsion, he would have occupied himself permanently with art, in a leisurely way, rather than with science. It is my surmise that his career may have been determined to some extent by that of his contemporary and friend Edward C. Pickering, whose natural bent for science was unmistakable. The two graduated from the Lawrence Scientific School of Harvard University in the same year, 1865, with the degree of S. B. Each taught mathematics for two or three years at Harvard; each was afterward for a time in the physics department of the Massachusetts Institute of Technology; each returned finally to the service of Harvard, Trowbridge as assistant professor of physics in 1870,

Pickering as professor of astronomy in 1876; for about forty years they were near neighbors in Cambridge. Pickering was, I think, the first American to write a laboratory manual of physics. Trowbridge was one of the first, if not the very first, of Americans to put students into the way of original research in physics.

It was his function to bring Harvard over from its old habit of set lectures, demonstrations, and strict textbook instruction, to the new habit of laboratory practice, research, and constructive thought. I well remember my first meeting with him, in 1877, when I was on my way to become a student under Rowland at Johns Hopkins. The routine physics instruction of that time was given in Harvard Hall, one of the older buildings in the "Yard," and the main body of the physics apparatus was housed there, as it continued to be for several years more; but Trowbridge had, for purposes of laboratory instruction and research, got possession of certain rooms in Lawrence Hall, domicile of the Lawrence Scientific School, then almost defunct. He had fitted up the main room on the first floor with large work tables, and it was there that I first saw him. My recollection of that first meeting presents him, a slim, graceful figure with thoughtful face and finely modelled head, standing beside one of these tables, on which was a certain electrical measuring instrument, such as none of his elders or predecessors at Harvard would ever have had use for, or perhaps understood.

I remember nothing else in the room and I think there was little else to be seen there. I was looking at the very nucleus, one man and one instrument, of the great laboratory of physics research which exists today at Harvard and of which all Harvard men should be proud. Trowbridge, more than any other man, was the creator of this laboratory. His own personal achievements in research were not so extensive as those of some other men, his juniors and mine, to whom he gave the opportunities they still enjoy; but he had the spirit of progress, he had a vision and constructive ability, he recognized and gave scope to the capabilities of other men.

For example, many years ago he planned, and employed the laboratory janitors to construct, a storage battery of twenty thousand small cells, housed in a great loft which had found no other use. For years not very much came of this installation, but after a time the course of research took such a turn that men had to wait for their opportunity to use it in rotation. The story of the "constant temperature room," at the base of the tower in the west wing, is somewhat the same. I don't know that Trowbridge ever did anything in this room, but he planned it and had it constructed. I used it for a year or so in experiments, on falling bodies, which attracted some attention, and Sabine made it famous by carrying on there some of his fundamental researches in architectural acoustics.

The earliest scientific publication by Trowbridge that has come to my attention is an account of a cosine galvanometer, devised by himself, in the *American Journal of Science* for 1871. He was an associate editor of this *Journal* for many years and made numerous contributions to its pages, including short notices of work done by others.

Another much used medium of publication for work done by himself or his students was the *Proceedings of the American Academy of Arts and Sciences* in Boston. His earliest paper in these *Proceedings*, presented, apparently, on January 9, 1872, dealt with the sources of error in attempts to show "the existence of electric currents in nerve and muscle." This paper, about 3 pages long, described experiments by the author and referred to the works of DuBois-Reymond and Matteucci. In volume 10 (1874-75) of the *A. A. Proceedings* he had a paper of about 4 pages "On a New Induction Coil." It describes experiments on the advantage to be gained by providing the ordinary straight core of the coil with an armature. "The iron core, with the armature, would then be in the form of a hollow square, one side of which is made up of a bundle of fine iron wires and the remaining three sides constitute the armature." The same volume gives evidence that Trowbridge's zeal for research was exerting a stimulating influence on his pupils at Harvard, for it contains

research papers by two undergraduates, members of the class of 1876, B. O. Peirce and Lefavour.

Volume 11 (1875-'76) contains a paper in continuation of work on armatures for electro-magnets and another, of 3 pages, "On the So-Called Etheric Force," in discussion of a phenomenon, discovered in Edison's laboratory, which attracted considerable transient attention. This volume has also a paper, written by B. O. Peirce, describing work done under Trowbridge's influence. It is "On the Induction Spark Produced in Breaking a Galvanic Circuit Between Poles of a Magnet." This is a remarkable production for its time and place, and I have commented upon it at length in my Biographical Memoir of Peirce.

In volume 12 we find, by Trowbridge, a paper of 6 pages "On Vortex Rings in Liquids." It describes the rings formed by dropping liquids into liquids and makes a considerable display of mathematics.

These details, even if none of the papers mentioned proved to be of any great permanent importance, seem to me well worth recording, for they show conclusively the existence of a research atmosphere and the initiation of a research habit in the department of physics at Harvard a year or two before Johns Hopkins had opened its door for students.

In Volume 14 (1878-'79) is a paper, 8 pages long, appropriate to the time when dynamo-electric machinery was in process of rapid evolution, "On the Heat Produced by the Rapid Magnetization and Demagnetization of Magnetic Metals." Co-author with Trowbridge was Walter N. Hill of the U. S. Torpedo Station at Newport, R. I., and the research was probably carried on at Newport because of the dynamo equipment of the torpedo station. Iron, cobalt and nickel were studied, the "fine specimens" of the latter metals having been furnished by Mr. Joseph Wharton of Philadelphia. The work described was quantitative and careful. Currents, whether direct or alternating, were measured by means of an electro-dynamometer, constructed in general accord with the design of the apparatus described in Maxwell's "Electricity and Magnetism," Art. 725, but with some features which were, I think, due to Trowbridge.

Thus, the current did not traverse the wires or threads by which the inner coil was suspended, but entered and left the movable part of the instrument by means of mercury contacts which were cooled by running water. By means of this arrangement the apparatus was adapted to the measurement of fairly strong currents, which at that time were recorded in "webers per second," substantially the equivalent of present day amperes. Trowbridge had considerable facility in the design of apparatus, being endowed with that sense of spatial relations which goes with ability in free-hand drawing. Some years later, in volume 18 (1882-'83) appeared another paper by the same authors on the same general subject, the experiments now being limited to iron and steel. The conclusion drawn is "that heat developed by reversals of magnetization is probably due to induced currents, and not to molecular vibrations." The authors were doubtless correct as to the main portion of the heating they observed, but they overlooked or failed to make sure of the genuine hysteresis-loss of energy.

Going back to volume 14 we find another paper by Trowbridge, "Methods of Measuring Electric Currents of Great Strength; Together With a Comparison of the Wilde, the Gramme, and the Siemens Machine." It is in this paper that the electro-dynamometer, commented upon above, is described in detail with drawings. The dynamos, "dynamo-electric engines," tested were those of the Torpedo Station at Newport. Harvard, of course, had no dynamos, except those of a crude lecture-room type, at this time.

In Volume 15 (1880) we have "Simple Apparatus for Illustrating Periodic Motion," a short paper, with figures, and another on "Illustration of the Conservation of Energy." The latter paper discusses briefly, from the point of view indicated by its title, experiments, already referred to (volume 14), on heat generated by magnetization and demagnetization. It is interesting to note that in the original paper describing these experiments the term "conservation of force" is used. This was about 1878.

In volume 16 (1880-'81) we have from Trowbridge "The Earth as a Conductor of Electricity." This paper describes interesting experiments on the detection, by means of a telephone with grounded terminals, of time-signals sent out from the Harvard Astronomical Observatory. The conclusions are:

"1. Disturbances in telephone circuits usually attributed to effects of induction are, in general, due to contiguous grounds of battery circuits. A return wire is the only way to obviate these disturbances."

"2. The well-defined equipotential surfaces in the neighborhood of battery grounds show the theoretical possibility of telegraphy across bodies of water without the employment of a cable, and lead us to greatly extend the practical limit set by Steinheil."

"3. Earth currents have an intermittent character, with periods of maxima and minima, which may occur several times a minute during the entire day."

This was some years before the advent of electric street railways.

Volume 17 (1881-'82) contains no papers by Trowbridge but two serious ones by Charles Bingham Penrose of Philadelphia, who was working as a Harvard student in consultation with Trowbridge. The titles are: "Thermo-Electricity,—Peltier and Thomson Effects," "Thermo-Electric Line of Copper and Nickel Below 0°." Penrose, after taking the degree of Ph. D. at Harvard, quit the field of pure science and followed his distinguished father in taking up the study of medicine, which he was reported to find "easy" in comparison with research in physics. At any rate, he soon rose to eminence in his hereditary profession, though he never attained the popular fame of his brother, the Senator from Pennsylvania.

Volume 18 (1882-'83) contained a paper, already mentioned, on heat of magnetization and demagnetization. Volume 19 (1883-'84) had no paper from Trowbridge, who at this time was occupied with money-raising and plans for the Jefferson Physical Laboratory. Moreover, he was writing a book, intended for use in schools, "The New Physics," which appeared in 1884.

In Volume 20 (1884-'85), with Austin L. McRae, afterward Director of the School of Mines at Rolla, Mo., Trowbridge wrote on "The Effect of Temperature on the Magnetic Permeability of Iron and Cobalt," the paper being dated from the new laboratory. Another paper in the same volume was on "A Standard of Light," describing experiments on a strip of incandescent platinum as a standard light-source.

In the year 1886-'87 Professor C. C. Hutchins, on leave of absence from Bowdoin College, worked with Trowbridge, and the results of this partnership appear in two papers published in 1887-'88. This was one of the best working combinations that Trowbridge ever formed. Trowbridge had vision and enterprise; Hutchins contributed the skill of a master craftsman together with a habit of uncompromising thought and forthright speech. The title of the first paper is "Oxygen in the Sun." It examines by means of new experiments the evidence offered by Doctor Henry Draper and by Professor John C. Draper, respectively, tending to prove the presence of oxygen in the sun. One of these two distinguished authorities thought he had found coincidence between lines of the oxygen spark spectrum and certain bright lines in the solar spectrum. The other thought he had found coincidence with dark lines of the solar spectrum. Trowbridge and Hutchins reach a positive conclusion that neither of these investigators was right, though they are careful to state that they do not claim to prove the non-existence of any oxygen lines in the solar spectrum. Their finding relative to the work of the Drapers has been definitely accepted. It is, I believe, only within the last fifteen years that the presence of oxygen in the sun has been demonstrated.

The other paper is "On the Existence of Carbon in the Sun." The authors thought that they had found conclusive evidence of this existence; but I believe that, although carbon is now known to be present in the sun, it is doubtful whether at the date of their paper the theory of spectra was sufficiently developed to enable them to establish their conclusion.

In the same volume appears the first paper in which Trowbridge and Sabine collaborated, "Wave-Lengths of Metallic

Spectra in the Ultra Violet." Lines of metallic spectra produced in the laboratory by different methods, the arc method and the spark method, respectively, did not agree exactly in place with each other. The arc lines apparently coincided with the corresponding lines in the solar spectrum, but "when the electric spark with a large battery of Leyden jars was substituted for the electric arc, and the metallic lines obtained by the light of the spark were compared with those from the arc, occasionally a small displacement could be observed." After some speculation and experimentation we have the following: "It was difficult to believe that the displacement could arise from the noise of the spark. We believe, however, that it can be ascribed to this cause, and that the wave-lengths of metallic lines produced by burning metals in the electric arc or by vaporization in the electric spark are to one hundredth of a wave-length the same as those of the corresponding lines in the sun."

This statement is somewhat halting, but I believe that the main conclusion is in accord with present-day observation, which indicates that the difference in question may exist but must be very small.

This same volume 23 has another paper by Trowbridge and Sabine, "Selective Absorption of Metals for Ultra-Violet Light." One passage, perhaps the most significant, runs thus: "Here was a complete experimental proof that color [of the metal surface] in no way influences the selective absorption of metals for the ultra violet rays; for the copper mirror, which gave a strong yellow light by reflection, was as (sic.) capable of reflecting light of as short wave-length as the brilliant white surface of polished silver."

In volume 24 (1888-'89) Trowbridge collaborates with Samuel Sheldon, afterward President of the American Institute of Electrical Engineers, in two papers. The first is on "Neutralization of Induction," with especial reference to the protection of telephone circuits from disturbances due to neighboring electric railway systems, which were now rapidly coming into existence. "The best remedy for these disturbances is doubtless the adoption by either the power companies or the telephone

companies of entire metallic circuits in which the earth plays no part. If this is not possible, a system of neutralization for the inductive disturbances might be adopted as follows:" etc.

The second paper was on "The Magnetism of Nickel and Tungsten Alloys."

Volume 25 (1889-'90) has a paper by Trowbridge and Sabine, "Electrical Oscillations in Air." It describes experiments on the oscillations of spark in the discharge of an air condenser. The "conclusions" begin thus:

"1. The electrical oscillations in the air between the plates of an air condenser shows a periodicity [the nature of this periodicity is not well described in the paper] extending through the entire range of the oscillations. We believe this periodicity is the analogue of the phenomenon of Hysteresis in Magnetism," etc.

This conclusion has not been sustained by subsequent observations, but it would be unfair to dismiss the paper with this remark. The research which it describes was one of great experimental difficulty and to compare the actual time of oscillation with the theoretical, in such a way as to attain a close approach to accuracy, was no small achievement. A revolving mirror was used to throw the light of the spark upon the photographic plate, and such was the speed of this mirror, and its distance from the plate, that the whirling spot of light was moving about a mile a second when it fell upon the recording surface. It is easy to believe the quiet statement of the paper that adjustment, timing, of the spark so as to make the revolving light-beam hit the distant target was a difficult operation. More than one revolving mirror burst under the centrifugal strain and, to protect life and limb of the observers, the axis of revolution was made horizontal, so that the movement of the mirror fragments, in case of explosion, would be confined to a vertical plane.

In the same volume is a paper, by Trowbridge alone, on "Motion of Atoms in Electrical Discharges." "The conclusion seems to be a strong one, that the electrical oscillations do not carry the atoms of metals with them in spark discharges."

In volume 26 (1890-'91) Trowbridge describes experiments on "Dampening of Electrical Oscillations on Iron Wires." Sa-

bine's name does not appear at the head of this paper, but we find on page 23 this passage: "I wish to express my deep obligations to my assistant, Mr. W. C. Sabine, for his valuable suggestions and for his skill in the mechanical details of this investigation." I believe that Trowbridge intended and wished to have Sabine named as co-author of the paper; but Sabine for some reason was unwilling to approve this arrangement and he never, so far as I know, collaborated with anyone in publication after this.

For several years in the last decade of the nineteenth century Trowbridge published little or nothing in the American Academy "*Proceedings*," but he was not inactive. Undertakings of a somewhat literary character must have engaged much of his attention at this time, for, as the appended bibliography will show, he produced a number of books, of a popular character, in rapid succession. He had undubitable equipment for productions of this sort, inventive imagination, an easy-going pen and a facile pencil. None of these books created any great excitement, but their very number shows that they must, as a whole, have had a considerable public. But literary production can hardly have been his chief occupation during this prolific period, for, with various collaborators he now published numerous papers in the *American Journal of Science*.

In fact, though frequently research papers published by Trowbridge in this *Journal* had been reprints or abstracts of articles appearing first in the Academy "*Proceedings*," there were, as the bibliography will show, some, from 1871 on, that went directly to the *Journal*.

There was in the early 90's considerable doubt and discussion as to whether the magnetic property of iron is effective with rapid alternations of electric current along the metal. Hertz and also Lodge, on experimental evidence of a not very precise character, had given a negative answer to this question, and I believe that Stefan had supported this conclusion on theoretical grounds. As we have already seen, Trowbridge had in 1891 published in the Academy "*Proceedings*" a paper on the "Dampening of Electrical Oscillations on Iron Wires," and in 1894 he

printed in the *American Journal* an account of his experiments on "Change of Period of Electrical Waves on Iron Wires." The alternations here studied were of such frequency as may occur in the discharge of a Leyden jar. About the same time, and apparently at Trowbridge's suggestion, Charles E. St. John was working in the Jefferson Laboratory on the same general subject by a different method. The results of the two studies were published in the same number of the *Journal*, and Trowbridge remarks, with characteristic courtesy, "My results confirm those of Mr. Charles E. St. John, who has shown by an entirely different method that the wave-lengths sent out by a Hertzian vibrator on iron wires differ in length from those transmitted on copper wires of the same geometrical form as the iron wires."

Soon after this Trowbridge and Duane, using, with some modifications, the apparatus of Hertz, measured the velocity of electric waves along wires. Their results were published in vols. 49 and 50 (1895) of the *Journal*. In the second paper the "average value of velocity" thus found is given as 3.0024×10^{10} cm. per second, the "generally accepted value for velocity of light" being at that time 2.998×10^{10} cm. per second.

Volume 3 (1897) of the *Journal*, 4th Series, contains a number of papers by Trowbridge in collaboration with T. W. Richards. The first is on "The Spectra of Argon," a study for which Lord Rayleigh furnished the material, "a portion of the purest preparation which had been used in his final determination of the density of the gas." The conditions under which the "red glow" and the "blue glow," respectively, occurred were carefully examined, by use of the Trowbridge storage battery consisting at this time of 5,000 cells. The second paper dealt with the multiple spectra of certain other gases, helium, hydrogen, nitrogen, and the vapor of iodine, the storage battery used having now 10,000 cells. The third paper is on "The Temperature and Ohmic Resistance of Gases during the Oscillatory Electric Discharge." This was before the recognition of atomic disintegration and the consequent existence of free electrons. The authors found that "a mass of gas at low tension contained in a capillary tube may act as though it opposed a resistance of only

five or six ohms to the spark of a large condenser," but their speculations as to the causes of the phenomena observed did not, I think, go beyond the suggestion of disintegration of the gas molecules into free atoms. The last of the papers in which Trowbridge and Richards collaborated gave the results of experiments on the resistance of electrolytes to oscillatory electric discharges. They found that electrolytes, in contrast with gases, do not show any great change of conductivity with great change in the intensity of current.

In 1897 Trowbridge began to publish again in the American Academy "*Proceedings*" and for some ten or twelve years thereafter papers of research or speculation appeared under his name in both these "*Proceedings*" and the *American Journal of Science*.

Volume 32 (1896-'97) of the "*Proceedings*" contains "The Energy Conditions Necessary to Produce the Röntgen Rays." This gives a somewhat detailed account of the great storage battery, consisting now of 10,000 small cells, which had been used in many of the researches already mentioned and was a unique feature of the Jefferson Physical Laboratory equipment. This resource was due entirely to Trowbridge's initiative, foresight, and capacity for seeing the possibilities in other men; for it was constructed by employees of the laboratory, no one of whom, except a carpenter, was initially a skilled workman. This battery was used for producing the Röntgen rays, by means of the discharge of condensers.

A year later we find "An Inquiry into the Nature of Electrical Discharges in Air and Gases." This paper describes further experiments with the storage battery. The "conclusions" begin thus: "Beyond one million volts the initial resistance of atmospheric air to electrical discharges becomes less and less, and in certain conditions can be as low as one thousand ohms between terminals two or three inches apart." Of course, this still was before everybody knew about ionization of the gases in electrical discharge. Trowbridge was here occupied with an important subject, even if he did not completely penetrate its mysteries. Here, as in reviewing other undertakings of his, I am reminded

of a remark I once heard from Rowland concerning some venture of his own: "If we succeed, we shall have made a great discovery. If we fail, we shall have tried to make a great discovery."

In volume 38 (1902-'03) of the "*Proceedings*" we have a paper of about eight pages on the "Spectra of Gases and Metals at High Temperatures," which contains but little of a conclusive character. The next two volumes contain no papers by Trowbridge, but the latter, volume 40, gives a paper "Resonance in Wireless Telegraphy," by G. W. Pierce, which may well be mentioned here, as Pierce began his research work at Harvard under Trowbridge's immediate direction.

"Slow Moving Electrical Luminous Effects" is the title of a paper in volume 41. This refers to work of Righi on the same subject, made by means of condenser discharges, whereas Trowbridge uses his storage battery directly, "without the interposition of a spark gap," to send discharges through tubes of rarefied gases. Mention is made of the "ionization theory of Townshend" as giving the best explanation of the effects observed, which Trowbridge thought might be related to the phenomenon of "ball lightning."

In 1907-'08 he was occupied with a "Longitudinal Magnetic Field and the Cathode Rays" and with "Positive Rays." In the first case he thought that the use of a magnetic field "has the advantage of producing the X-rays from a sharper focus and should, therefore, give better definition." In the latter his hope was "to measure the group velocity of the positive rays by producing a standing wave, or a stratum of maximum collisions in the space between the anode and the cathode."

In volume 45 (1909-'10) of the "*Proceedings*" we have "Discharges of Electricity Through Hydrogen." The first "conclusion" runs thus: "The striæ in Geissler tubes are analogous to waves set up in narrow channels by opposing pulsations of different periods."

I think that this was the last research paper published by Trowbridge, though in later years he made occasional informal

communications at meetings of the American Academy of Arts and Sciences, of which he was President from 1909 to 1916.

For many years the *Philosophical Magazine* reprinted most of the research papers published in America by Trowbridge and his collaborators, copies in manuscript or in advance proof having been furnished in order to achieve approximate simultaneity of appearance in the different publications. I have discovered only two or three papers of Trowbridge in the *Philosophical Magazine* which are not duplications of those published in America.

In the early days of commercial development of the telephone Trowbridge was professionally consulted, and then or later he took out a number of patents relating to a telephone operation. He also, about 1895, was interested in a certain form of storage battery and secured or applied for a patent on a process for renovating such batteries. I am not aware that any of his patents proved to be commercially valuable.

It is a curious fact that Trowbridge, though he had so important a part in the construction and operation of the Jefferson Physical Laboratory, and though he was the chief instigator of its research activities, was not its first Director. Professor Joseph Lovering, a strongly marked personality of the old-fashioned professorial type, had this title from 1884 till his retirement, in 1888. This arrangement was a somewhat anomalous one, for I doubt whether Lovering, excellent lecturer and careful man of business as he was, ever performed a research experiment in the whole of his long life, including fifty years' occupancy of a full professorship at Harvard. When Trowbridge did become the Director, in 1888, the effect, so far as provision for research was concerned, was speedily apparent.

Trowbridge's habitual manner, outside the circle of his few real intimates, was quiet, gentle, and perhaps slightly depressed. He was courteous, generous, and possibly a little too yielding, but when he became aware of intentional opposition his sensitive nature was likely to feel aggrieved and then he could be tenacious. Moreover, he had a sense of humor which, though ordinarily concealed, could not safely be ignored. Things

which he had seen or heard with an air of sadness he would afterward relate with tears of hilarity flooding his eyes.

He married, in 1875, Mrs. Mary Louise Gray (née Thayer), who died in 1907. There were no children of this marriage, but a step-daughter, Miss Alice Gray, afterward Mrs. Edmund M. Parker of Cambridge, was like an own daughter to him, both before and after the death of her mother. After his retirement from the Rumford Professorship he lived through several pen-
sive and perhaps melancholy years, occupying himself with his rose garden and with that unfailing subject of human interest, the life work of Benjamin Franklin, especially with its scientific aspects. Fortunately he had, in his later years, ample financial means. He died February 18, 1923.

Two attempts have been made to put into one brief statement a summary of Trowbridge's character and life-work. One of these, written as the dedication for a volume of scientific research papers issued by the Jefferson Physical Laboratory about the time of his retirement, in 1910, has appeared more than once in print. The other, inscribed on a memorial tablet in the main lecture-room of the Laboratory, is this:

JOHN TROWBRIDGE
1843-1923
GENIAL AND GIFTED GENTLEMAN
LOVER OF SCIENCE AND ART
ONE OF THE FIRST TO INDUCT
AMERICAN COLLEGE YOUTHS
INTO THE DEVOTED FRUITFUL AND
BENEFICIENT LIFE OF EXPERIMENTAL
RESEARCH—LEADING SPIRIT IN THE
WORK OF FOUNDING EQUIPPING
AND DIRECTING THIS
PHYSICAL LABORATORY

PAPERS

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On a New Induction Coil. 10 (1875), pp. 381-384.

On the Effect of Thin Plates of Iron used as Armatures to Electro-Magnets; On the So-Called Etheric Force; On a New Form of Mirror Galvanometer. 11 (1876), pp. 202-209.

On Vortex Rings in Liquids. 12 (1877), pp. 131-136.

On the Heat Produced by the Rapid Magnetization and Demagnetization of the Magnetic Metals (with W. N. Hill). 14 (1878), pp. 114-121.

Methods of Measuring Electric Currents of Great Strength, together with a Comparison of the Wilde, the Gramme, and the Siemen's Machines. 14 (1878), pp. 122-132.

Simple Apparatus for Illustrating Periodic Motion. 15 (1880), pp. 232-234.

Illustration of the Conservation of Energy. 15 (1880), p. 235.

The Earth as a Conductor of Electricity. 16 (1880-81), pp. 58-62.

On the Heat Produced in Iron and Steel by Reversals of Magnetization (with W. N. Hill). 18 (1883), pp. 197-204.

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Influence of Magnetism upon Thermal Conductivity (with C. B. Penrose). 18 (1883), pp. 210-213.

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The Electromotive Force of Alloys (with E. K. Stevens). 18 (1883), pp. 221-225.

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Oxygen in the Sun (with C. C. Hutchins). 23 (1887), pp. 1-9.

On the Existence of Carbon in the Sun (with C. C. Hutchins). 23 (1887), pp. 10-13.

Wave Lengths of Metallic Spectra in the Ultra Violet (with W. C. Sabine). 23 (1888), pp. 288-298.

Selective Absorption of Metals for Ultra Violet Light (with W. C. Sabine). 23 (1888), pp. 299-300.

- Neutralization of Induction (with Samuel Sheldon). 24 (1889), pp. 176-180.
- The Magnetism of Nickel and Tungsten Alloys (with Samuel Sheldon). 24 (1889), pp. 181-184.
- Electrical Oscillations in Air (with W. C. Sabine). 25 (1890), pp. 109-123.
- Motion of Atoms in Electrical Discharges. 25 (1890), pp. 192-194.
- Dampening of Electrical Oscillations on Iron Wires. 26 (1891), pp. 115-123.
- The Energy Conditions Necessary to Produce the Röntgen Rays. 32 (1897), pp. 253-265.
- An Inquiry into the Nature of Electrical Discharges in Air and Gases. 33 (1898), pp. 433-452.
- The Spectra of Gases and Metals at High Temperatures. 38 (1903), pp. 679-688.
- Electrical Discharges: 1. Slow Moving Electrical Luminous Effects. 2. Magnetic Field and Electric Discharges. 41 (1906), pp. 627-644.
- Longitudinal Magnetic Field and the Cathode Rays. 43 (1908), pp. 397-404.
- Positive Rays. 43 (1908), pp. 511-517.
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Published in the American Journal of Science

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- Electromotive Action of Liquids Separated by Membranes. 3d series, 3 (1872), pp. 342-346.
- Electrical Condition of Gas Flames. 3d series, 4 (1872), pp. 4-7.
- Ohm's Law Considered from a Geometrical Point of View. 3d series, 4 (1872), pp. 115-117.
- Induced Currents on Derived Circuits. 3d series, 5 (1873), pp. 372-373.
- Freeing a Magnetic Bar from the Influence of the Earth's Magnetism. 3d series, 7 (1874), pp. 490-493.
- Molecular Change Produced by the Passage of Electrical Currents Through Iron and Steel Bars. 3d series, 8 (1874), pp. 18-21.
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- Use of Steam in Spectrum Analysis (with W. C. Sabine). 3d series, 37 (1889), pp. 114-116.
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- A Phasemeter. 3d series, 43 (1892), pp. 232-234.
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- Triangulation by Cathode Photography. 3d series, 51 (1896), pp. 245-246.
- Carbon and Oxygen in The Sun. 4th series, 1 (1896), pp. 329-333.
- Spectra of Argon (with T. W. Richards). 4th series, 3 (1897), pp. 15-20.
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- Temperature and Ohmic Resistance of Gases during the Oscillatory Electric Discharge (with T. W. Richards). 4th series, 3 (1897), pp. 327-342.
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- Electrical Discharges in Air. 4th series, 4 (1897), pp. 190-193.
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- Phosphorescence Produced by Electrification (with J. E. Burbank). 4th series, 5 (1898), pp. 55, 56.
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- The Source of X-Rays (with J. E. Burbank). 4th series, 5 (1898) pp. 129-134.
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NATIONAL ACADEMY OF SCIENCES
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OF

IRA REMSEN

1846-1927

BY

WILLIAM ALBERT NOYES

and

JAMES FLACK NORRIS

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1931

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BY WILLIAM ALBERT NOYES AND JAMES FLACK NORRIS

Two men, Ira Remsen and Wilhelm Ostwald, stand out during the last fifty years as great teachers and as founders of chemical journals which have had a profound influence on the development of chemistry. In these two respects their work is comparable with that of Liebig during the middle of the nineteenth century.

Ira Remsen was for many years the outstanding figure in American chemistry. When the history of the development of the science in this country is written, the fact will be evident that through his influence the serious study of chemistry and the output of new knowledge very rapidly increased. Much had been accomplished by a few gifted men in America before Remsen's day, but he opened up a life work in chemistry as a career to many, and developed a spirit of research that spread over the country.

He made it possible for a young man to be adequately and broadly trained at home, whereas Remsen himself and others who sought, at that time, to prepare themselves for work in chemistry had been forced to go to Europe. And what is equally important, he transplanted the "atmosphere" of the laboratories of the great masters—the spirit of hard work, the desire to learn and a love of chemistry.

Ira Remsen was born in New York City, February 10, 1846. His parents were both descended from the early Dutch settlers of New York and his mother had also Huguenot blood in her veins. For two years, from eight to ten, the boy lived in the country and had that intimate contact with nature which is impossible for a lad who spends his life exclusively in a city. A part of his early education was received in country schools. After further study in the public schools of New York City he entered the Free Academy, now the College of the City of New

¹ The first part of this Memoir is chiefly a combination of biographical sketches which appeared in *Science*, 66, 243 (1927); *J. Chem. Soc.*, 1927, 3182 and *J. Amer. Chem. Soc.*, *Proceedings* 1928, p. 67.

York, where he studied Latin, Greek, mathematics, history and a very little science. He did well in Latin and Greek and it was doubtless during those years that he laid the foundation for that perfect command of accurate English which has made it such a delight to read his books and to listen to his lectures. His interest in science seems to have been awakened at this period by the popular, illustrated lectures given by Dr. Doremus at the Cooper Institute.

He did not, however, complete the four years of work required for graduation at the Free Academy. Many years later he received the bachelor's degree from the College of the City of New York, as of the class of 1865. He was accustomed to say, with some pride, that he was one of the few men who had received the rank of M.D. from the College of Physicians and Surgeons without having received the bachelor's degree. He also said, at one time, that he thought he was the only university president in America who had not completed a four years' college course.

After a few years in the Free Academy, Remsen's father decided that he should become a physician and apprenticed him to a doctor who taught in a homeopathic medical college.

Benjamin Harrow in his "Eminent Chemists of our Time" tells us that in one of his addresses Remsen recalled an incident of this time.

"While reading a text-book of chemistry I came upon the statement, 'nitric acid acts upon copper'. I was getting tired of reading such absurd stuff and I determined to see what this meant. Copper was more or less familiar to me, for copper cents were then in use. I had seen a bottle marked 'nitric acid' on a table in the doctor's office where I was then 'doing time!' I did not know its peculiarities but I was getting on and likely to learn. The spirit of adventure was upon me.

"Having nitric acid and copper, I had only to learn what the words 'acts upon' meant. Then the statement, 'nitric acid acts upon copper' would be something more than mere words. All was still. In the interest of knowledge I was even willing to sacrifice one of the few copper cents then in my possession.

"I put one of them on the table; opened the bottle marked 'nitric acid'; poured some of the liquid on the copper; and prepared to take an observation. But what was this wonderful thing I beheld? The cent was already changed, and it was no small change either. A greenish blue liquid foamed and fumed over the cent and over the table. The air in the neighborhood of the performance became colored dark red. A great colored cloud arose. This was disagreeable and suffocating. How should I stop this?

"I tried to get rid of the objectionable mess by picking it up and throwing it out of the window, which I had meanwhile opened. I learnt another fact—nitric acid not only acts on copper but it acts on fingers. The pain led to another unpremeditated experiment. I drew my fingers across my trousers and another fact was discovered. Nitric acid acts upon trousers.

"Taking everything into consideration, that was the most impressive experiment, and, relatively, probably the most costly I have ever performed. I tell it even now with interest. It was a revelation to me. It resulted in a desire on my part to learn more about that kind of action. Plainly the only way to learn about it was to see its results, to experiment, to work in a laboratory."

It is evident that Remsen learned very little chemistry in the homeopathic college but his preceptor evidently thought well of him and made him lecture assistant and quiz instructor in the college.

He soon revolted at the inefficient instruction, however, and induced his father to send him to the College of Physicians and Surgeons of Columbia University. In 1867, at the age of 21, he graduated and was supposed to be ready for the practice of medicine. In 1878, in an address before the Medical Faculty of Maryland, he said:

"Eleven years ago, in company with 90 others, I was proclaimed fit to enter the career of a medical man. My erudition in medical matters was exhibited in a thesis on the *Fatty Degeneration of the Liver*, a subject on which I was and am profoundly ignorant. I had in fact never seen a liver which had

undergone fatty degeneration, nor a patient who possessed, or was supposed to possess one; nor, I may add, have I had that pleasure up to this day."

"And yet Remsen got one of the two prizes offered for the best theses!"

After graduation he was offered a desirable partnership with a well-known physician but once more he refused to be guided by the wishes of his father and set out for Germany to study chemistry.

Liebig's name had attracted him to Munich and he had not learned that the great master had given up the direction of students some time before and had gone to the Bavarian University with the understanding that he could devote his time to his own studies and writing and that his duty should consist in giving a single course of lectures in inorganic chemistry. Remsen was forced to study with an able *Privatdocent*, Jacob Volhard. From him he received his first systematic laboratory instruction. Before that he had never performed the simplest analysis. Thorough training in analytical chemistry was, at that time, considered to be the only routine laboratory work necessary for the preparation of a chemist to begin research, and we may be sure that the fundamental basis for his career was well laid during this year of intimate association with Volhard.

During the summer of 1868, Wöhler made one of his friendly visits to Liebig and through Volhard, Remsen was introduced to him and arranged to go to Göttingen in the fall. There he began research work under the direction of Fittig and two years later received his degree of Ph.D. at the age of twenty-four. When we remember that Remsen spent only one year in the systematic study of chemistry and two years in research in earning his degree, we are tempted to question whether the long years of routine instruction which are required of young chemists today do not tend to dim that eager enthusiasm and repress the initiative so invaluable for a successful career.

It does not follow, however, that because Remsen did not take the varied courses of routine lectures which we expect of students today he failed to become very thoroughly acquainted

with the chemistry of his times. He once told me that during his stay in Germany he read the volumes of Liebig's *Annalen*—150 volumes had been published in 1870—until he was acquainted with all the important papers published in that journal.

The title of his dissertation was, "Untersuchungen über die Constitution der Piperinsäure."

The same year that Remsen received his doctor's degree, Fittig was called to the professorship at Tübingen and he asked Remsen to go with him as his lecture and laboratory assistant. He continued in this position for two years and in this way, for five years in all, he drank in the spirit of the German laboratories.

It was a fortunate time for the eager, enthusiastic young man. In 1858 Canizzaro had shown the importance of Avogadro's principle and laid the foundation for a system of true atomic weights. The same year, Couper and Kekulé extended Frankland's doctrine of valence to explain the structure of carbon compounds, and hundreds of professors and students were working together, after the model of Liebig's laboratory, in the fascinating world of organic chemistry.

It was at Tübingen, too, that a young Scotchman rang at the door one day and asked in broken German, for the "Vorlesungszimmer." Remsen answered, "Oh! I guess you want the lecture room." So there was begun the life-long friendship with Sir William Ramsay. Only a few months before his death, Sir William wrote to Remsen, "Well, I am tired and must stop. I look back to my long friendship with you as a very happy episode in a very happy life; for my life has been a very happy one." When Remsen helped with the plans of the Kent Chemical Laboratory of the University of Chicago, he provided few rooms for isolated students and he made the remark that students learn more from each other than from their teachers. When two such students as Ramsay and Remsen met, we can well believe that this was true.

During the two years with Fittig at Tübingen he did work which contributed something toward clearing up the confusion

about the structure of derivatives of benzene occasioned by the wandering of hydroxyl groups when sulfonic acids are fused with caustic potash, a question of great importance at that time. The work was directly connected with his studies of piperic and piperonylic acids with Fittig, which formed the basis for his Doctor's thesis.

While at Tübingen he also undertook a study of the sulfo-benzoic acids and demonstrated that both the meta and para acids are formed by treating benzoic acid with fuming sulfuric acid, the meta acid being formed in larger amounts. In an endeavor to secure the para acid in greater quantities, he oxidized a mixture of o- and p-sulfotoluene and discovered that the former resists oxidation by the usual mixture of potassium dichromate and sulfuric acid. Fittig had supposed that ortho compounds are completely destroyed by such oxidation. This discovery was the starting point for Remsen's Law that groups in the ortho position "protect" methyl, ethyl and propyl groups in derivatives of benzene from oxidation by chromic or nitric acid. This conduct of ortho compounds must have suggested the possibility of steric hindrance but Remsen was very careful not to express this in his papers and the discovery, later, that potassium permanganate and other alkaline oxidizing agents readily oxidize such groups, has shown that steric relations tell only a part of the story.

Remsen also showed that the sulfamides of toluene and other hydrocarbons are easily oxidized by the chromic acid mixture without hydrolysis, giving difficultly soluble acids which crystallize well and furnish excellent compounds for the further study of the law. As will be seen below, nearly all of Remsen's most important work in organic chemistry grew, directly or indirectly, from this beginning.

Remsen returned to America in 1872 and, after some delay, was appointed professor of chemistry and physics at Williams College. When he assumed his duties he found no laboratory and scant encouragement to teach science other than as a small element of general "culture" in an old-fashioned classical college.

As an illustration of the spirit of the New England colleges of that day, the following incident related by Professor J. M. Kingsley is illuminating:

"In the autumn of 1874, together with the rest of the junior class in Williams College, I began the study of chemistry under Professor Ira Remsen. After a few days I asked him for the privilege of carrying my studies farther in his private laboratory, as there was no laboratory work connected with the regular course. He replied to the effect that he would have to lay my request before the faculty, as there was no provision for such work in the curriculum. A few days later he asked me to stop after the class was dismissed, and then he informed me, in the most disgusted tones, that 'The Faculty, *in their wisdom*, have decided that you would break too much glassware and waste too many chemicals to allow you to work in my laboratory.'"

Kingsley became a zoologist of note instead of a chemist.

Satisfactory textbooks are an important aid in teaching. No book that was available for American students adequately treated the fundamental concepts and theories on which the newer chemistry was based. The appreciation of the significance of Avogadro's hypothesis, molecular weights, specific heats, etc., and the developments based on the structure theory of organic chemistry, had recently placed chemistry on a firm theoretical foundation. Remsen undertook, while at Williams College, to weave the important principles of chemistry into a logical whole. The result was his "Theoretical Chemistry," in which such subjects as the determination of atomic weights from the weights of gases and vapors were explained in a masterful but simple way. He even included a statement of the demonstration of the monatomic character of the molecules of mercury vapor, based on the ratio between the specific heat at constant volume and that at constant pressure.

Remsen repeatedly expressed his admiration for the great achievement of Canizzaro in so interpreting the facts of chemistry in the light of Avogadro's hypothesis that chemists saw clearly for the first time the significance of the relationship. His own great achievement consisted in reducing this and other

fundamental principles underlying so-called theoretical chemistry to such a simple form that the student could understand the beauty of the logic of it all. The book received immediate recognition and was soon translated into German and Italian. His methods of presentation have been largely followed or slightly modified in the great number of elementary books that have been written since his masterpiece appeared.

During this period he also translated Wöhler's "Organic Chemistry."

Remsen's attempt to systematize and coördinate the fundamental principles of chemistry in his work on his textbooks had a marked effect on his subsequent research. While at Williams College he published an account of a research on the oxidation of carbon monoxide, which had as its aim the discovery of how the molecules involved reacted. He compared the unsaturation of the carbon in the oxide with that of the carbon atoms in ethylene. It was known that chromic acid oxidizes carbon monoxide. He reasoned that if the oxidation comes about as the result of atomic oxygen formed from the acid, or from oxygen molecules when oxygen gas is used, ozone should be capable of oxidizing carbon monoxide at the temperature at which the former is decomposed. He found, however, that no oxidation takes place. It had been stated that oxygen in the presence of phosphorus converts carbon monoxide to the dioxide, and the explanation put forward that ozone formed during the oxidation of the phosphorus caused the formation of carbon dioxide. Remsen determined to test this view and later carried out the work at Johns Hopkins University. His results were in accord with his previous experiments; no carbon dioxide was formed. The publication of his paper launched a controversy. The work was repeated with scrupulous accuracy. The outcome was the discovery that phosphorus, as ordinarily prepared, contains very minute amounts of carbon (up to 0.04%), and that the carbon dioxide observed by other observers came from this source and not from carbon monoxide. In the course of the investigation a new form of phosphorus was discovered and a method was developed for the analysis of phosphorus containing small amounts

of carbon. The heat of combustion of a mixture of carbon monoxide and oxygen shows that such a mixture is thermodynamically unstable and the mixture with ozone must be still more so. Chemists are almost as far from a theoretical explanation of these facts as they were when Remsen tried these experiments.

This research illustrates clearly with what skill he attacked a difficult problem involving the search for traces of a substance, the presence of which might be attributable to methods of manipulation or to unsuspected impurities. It showed his keenness in searching out and avoiding causes of error.

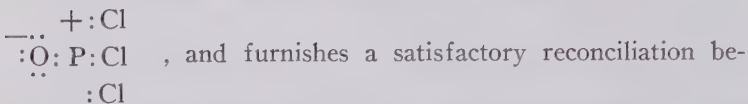
Remsen supplemented his study of the behavior of carbon monoxide and ozone with an investigation of the action of the latter on phosphorous trichloride. Both carbon and phosphorus appeared to be unsaturated. The carbon compound was not oxidized, but phosphorus trichloride was converted into the oxychloride. Remsen interpreted the reaction as one of true oxidation in which the valence of phosphorus increased from three to five. At that time the doctrine of constant valency was held by many and it was necessary to marshal the evidence in favor of his point of view. Phosphorus pentachloride had been explained as a so-called molecular compound of the trichloride and chlorine. Wurtz had recently shown that the vapor density of phosphorus pentachloride in an atmosphere of phosphorus trichloride approaches very close to the density corresponding to the formula PCl_5 and on the basis of this Remsen concluded that the struc-

ture of phosphorus oxychloride was
$$\begin{array}{c} \diagup \text{Cl} \\ \text{O}=\text{P}-\text{Cl} \\ \diagdown \text{Cl} \end{array} \quad . \quad \text{Kopp had}$$

shown much earlier that the doubly bound oxygen atoms of aldehydes and ketones has a much greater atomic volume than the singly bound oxygen atoms of ethers and alcohols. The increase in the atomic volumes should, of course, be ascribed to the double union and, in that sense, to both the oxygen and the carbon, as Sugden has done in his recent discussions. Thorpe had recently determined the molecular volume of phosphorus oxychloride and found that this indicated a singly bound oxygen

atom in the compound. He proposed the formula, $\begin{array}{c} \diagup \text{Cl} \\ \text{P}-\text{O}-\text{Cl} \\ \diagdown \text{Cl} \end{array}$.

Remsen, attaching more weight to Avogadro's principle than to the evidence based on atomic volumes, decided in favor of quinevalent phosphorus. Rather recently, Sugden has determined the parachor of phosphorus oxychloride and has confirmed Thorpe's evidence for a singly bound oxygen atom. He explains the relation, however, by assuming a semipolar union between the oxygen and phosphorus. This gives the formula,



tween the quinevalent phosphorus assumed by Remsen and the singly bound oxygen assumed by Thorpe.

The publication of the translation of Wöhler's "Organic Chemistry" and of the "Theoretical Chemistry" and, still more, Remsen's persistence in research under discouraging conditions, attracted the attention of President Gilman, who was seeking men for his faculty at the Johns Hopkins University. He had already secured Gildersleeve for Greek, Rowland for physics and Sylvester for mathematics. Remsen was invited to Baltimore to meet the Board of Trustees and was entertained at a dinner at which he was seated beside one member of the board after another. In this way Professor Remsen became one of that galaxy who worked with President Gilman to organize the first genuine university in America, where more than half the students were graduates of other colleges and where the purpose was not so much to teach what is already known as to develop men into productive scholars and add to the world's knowledge. President Gilman had the somewhat rare quality of fully trusting the men he selected and allowing them to develop the work of their departments without interference. His injunction to Remsen was, "Do your best work in your own way."

Professor Remsen followed rather closely the models with which he had become so familiar in Germany. He gave lectures on inorganic chemistry during the first semester and on organic

chemistry, the second. He illustrated these well with experiments and had a crystal-clear, masterful method of presenting his subject. Once a week there was a meeting of graduate students for reports on current literature.

Professor Morse has said of these "Journal Meetings": ". . . nowhere else [in America], so far as I know, had the advanced students been taken in and given an opportunity to familiarize themselves with the current progress of the science and of perfecting themselves in the art of giving concise and lucid expression to the information acquired in the course of their reading."

Of his lectures, Professor Morse says, "I will only say, as many others have said before me in effect, that I have never seen his equal as a master of simple and lucid exposition . . . as a teacher of many other teachers, his influence, direct and remote, has been and will continue to be of incalculable value to American students of chemistry."

In general, his method was that of the masters with whom he had come in contact in Germany, but he brought to bear on the problem a keen appreciation of the mental processes and needs of the student. His lectures were characterized by clearness of expression, the use of simple but effective logic, and the complete absence of dullness; they possessed an element of charm. Remsen avoided inconsequential details, but he inspired an appreciation of facts and the desire on the part of the student to search these out for himself. He brought out the high lights of a subject, emphasized principles, and made every lecture a model to be followed by the young, prospective teachers before him.

It was the custom for graduate students to follow closely his lectures given to beginners in chemistry and to attend the courses for graduate students as long as they were at work. Much could be learned by following the lectures on organic chemistry for the second or third time.

Remsen's lectures on the history of chemistry were a pure delight. He had come into close touch with many of the great men who had made modern chemistry, and he drew their pictures and appraised their influence in a fascinating way. Chem-

istry became a story of individual achievement in which there was still opportunity for one inspired with a desire to add to knowledge. In these lectures Remsen proved himself to be a philosopher as well as a scientist; and he often exhibited a sense of humor that was keen and delightful.

In the year in which one writer of this sketch received his Doctor's degree Professor Remsen called together the young men about to go out into the world. He talked for an hour on what was ahead of us; cautioned us against giving up the desire to push ahead by continued study and work. He warned us against allowing our present accomplishments to be the high spots in our lives. He urged us not to wait for a brilliant idea before beginning independent research, and emphasized the fact that Lavoisier's first contribution to chemistry was the analysis of a sample of gypsum. He told us that the fields in which the great masters had worked were still fruitful; the ground had only been scratched and the gleaner could be sure of an ample reward. He told us something of the methods he used in lecturing. A lecturer should be an historian rather than a prophet. It is less embarrassing to explain an unexpected result of an experiment after the observation has been made than it is to extricate oneself after telling what will happen and finding that it does not. (He evidently did not forget an experience he had when he was lecturing to an elementary class when the writer was a student. He told us that we should see that oxygen was a colorless gas. To his dismay and the amusement of the class the mercury oxide yielded, when heated, a distinctly yellow gas. A mistake of the lecture assistant was to blame.)

Professor Remsen cautioned us against giving dull lectures. The mind is capable of close application to a given subject for a limited time only. After a period requiring close attention the lecturer should see to it that a break occurs, that the mind is given a momentary rest. In every lecture the audience should have an opportunity to smile at least once. No extraneous commonplaces should be dragged in, but the resourceful lecturer will find some way to lighten the strain. This advice of

Remsen's was designed to be helpful; in it a thoughtful teacher spoke.

It is clear that the inspiration and training of future chemists held the highest place in Remsen's mind. He lived to see his efforts rewarded. Many of the leaders in research in chemistry today in America are the products of his care. The ability to express the principles of chemistry clearly and forcibly, acquired, consciously and unconsciously, by his disciples, has made many of them successful teachers.

The most important and vital part of his instruction was the daily visit to the desk of each research student. Often, at critical points, he would stop and work for minutes or for an hour or more with the student, and the product, in the end, was the joint work of professor and student as it had been in Liebig's laboratory.

Professor E. E. Reid writes: "It is impossible to characterize or describe Remsen. He had a keen sense of humor and a ready wit, a personality in the fullest sense of the term. He drew people to him but always kept them in their place."

Those trained under him look back to him as a father, who always required high quality in their work, who was wise in his advice and helpful in their difficulties.

Remsen extended his influence as a teacher by means of his textbooks. His "Organic Chemistry" was a masterpiece of lucidity and logic. It convinced the whole world that one could learn the fundamental principles of the science without being compelled to master the array of disjointed facts that were crowded together in the textbooks of the day when he produced his. The book received widespread recognition and was used over the world in translations in several languages. He wrote three textbooks on inorganic chemistry. The "Briefer Course" and "Introduction" remained for many years the standard texts on chemistry in America. He wrote, in all, eight textbooks and laboratory manuals.

Immediately after assuming his duties at Johns Hopkins University Professor Remsen began a series of interesting and important investigations, carried out with the aid of graduate stu-

dents and others. These were never undertaken merely for the purpose of adding to the list of organic compounds, which has grown all too rapidly in the hands of thousands of zealous chemists, young and old. There was always some fundamental principle at the basis of the work and his ideas were worked out with very great care and accuracy.

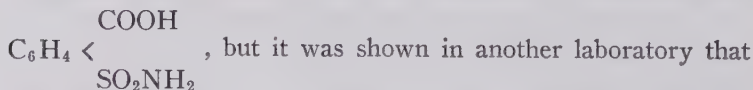
During the early years the starting point for many of the studies was the observations made at Tübingen that a methyl group ortho to a sulfonic acid group is not oxidized by the usual oxidizing mixture of potassium dichromate and sulfuric acid and that the sulfamide group, SO_2NH_2 , "protects" ortho alkyl groups in the same manner and is not hydrolysed during the oxidation. This principle is known as "Remsen's Law" and was extended to show that ethyl and propyl as well as methyl groups are "protected" from oxidation and that halogen atoms and nitro groups have the same effect as the sulfonic acid or sulfamide groups.

It was found, quite early, that ortho groups may be oxidized by potassium permanganate and Remsen and Fahlberg discovered that the oxidation of orthotoluene sulfamide,



was called benzoic sulfinide. This was the first of a whole series of similar compounds. Because of its extraordinary sweetness, several hundred times that of cane sugar, benzoic sulfinide is manufactured in large quantities and sold under the trade name of "saccharin."

In the earlier papers, it was assumed that the salts of the sulfinides were derived from the sulfamides of benzoic acid,



they are formed by the replacement of the hydrogen of the NH group or of the hydroxyl of a tautomeric form and that the true salts of the sulfamide of benzoic acid are not sweet.

The original method of preparing orthotoluene sulfamide, used for making benzoic sulfinide, is tedious and gives very poor yields. In the hope of preparing the sulfamide more easily, Remsen attempted to replace the amino group of the para-aminosulfamide of toluene with hydrogen by the well-known diazonium reaction. To his surprise, the amino group was replaced by the ethoxy group, $\text{O-C}_2\text{H}_5$, instead of being replaced by hydrogen.

Professor Reid reports an incident in the laboratory which also led to the study of this replacement reaction. The laboratory book said "add alcohol and smell the aldehyde." A student came to him and said he did not smell aldehyde. Remsen took the tube and could not smell aldehyde, either. He made this into a good story, telling how stubborn the student was who wouldn't smell aldehyde when told to do so.

These observations started a new series of interesting studies to determine conditions which cause the replacement by the ethoxy or methoxy group or by hydrogen. The first paper announcing the discovery was published in 1886. It was found that pressure has a marked effect on the reaction. The yield of the ethoxy derivative was 37% when the pressure was 120 mm. and 60% at 800 mm. The nature of the substituent and its position were found to be determining factors. The methyl radical in either the ortho, meta or para position brought about almost exclusively the replacement of the diazonium group by ethoxyl, whereas the nitro or carbonyl group in the three positions favored the replacement by hydrogen. Substitution in the para position favored the hydrogen reaction and in the ortho and meta positions the alkoxyl reaction. The proportions of the two compounds formed was thus shown to be determined by the character of the group and its position. The observations made are a valuable addition to our knowledge of the effect of substituents on linkages between atoms in organic compounds. They may prove helpful in the further development of the electronic theory of the nature of the bonds between atoms. They illustrate very clearly how far the experimental knowledge of organic chemistry has outrun the theoretical explanations at our command.

The commercial manufacture of "saccharin" soon made orthosulfobenzoic acid, $\text{C}_6\text{H}_4 < \begin{smallmatrix} \text{COOH} \\ \text{SO}_2\text{OH} \end{smallmatrix}$, more easily available.

In the course of the study of this compound the chloride was prepared. The oil obtained yielded on crystallization a solid possessing definite properties. Some years later in studying the action of aniline on the chloride the crude oil was used. Since two isomeric compounds were formed it appeared probable that the chloride was a mixture of isomers. It was suspected at that time that there were two chlorides derived from phthalic acid. The possibility of the existence of similar isomers in this case made the subject an important one to investigate. After several painstaking researches two crystalline substances were isolated from the oily chloride. One of these, the higher melting form first obtained, always gave a definite derivative when treated with reagents, the low melting chloride gave the two types of substances produced from the original mixture. The results indicated that the low melting chloride was unstable and rearranged in part to the other form when it reacted with other substances. In the midst of this work it was discovered by List and Stein that Remsen's low melting chloride was the eutectic mixture of the high melting variety he had isolated and the true isomer. This observation cleared up the situation and an intensive study of the two chlorides was undertaken. The high melting form of the chloride was shown to have the symmetrical structure,

$\text{C}_6\text{H}_4 < \begin{smallmatrix} \text{COCl} \\ \text{SO}_2\text{Cl} \end{smallmatrix}$, the low melting, the unsymmetrical structure,

$\text{C}_6\text{H}_4 < \begin{smallmatrix} \text{CCl}_2 \\ \text{SO}_2 \end{smallmatrix} > \text{O}$. The unsymmetrical chloride was found to be

the more reactive and to yield derivatives, the structure of which were shown to be in accord with the formula assigned to the chloride. For example, ammonia converted the unsymmetrical chloride into *o*-cyanobenzene-sulfonic acid, and the symmetrical chloride into saccharin.

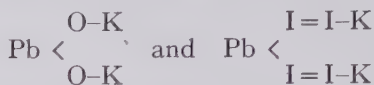
The isomerism exhibited in these two compounds and the difference in the behavior of the halogen atoms as the result of their position and relationship in the two molecules were facts that invited detailed study. The work was broadened by the investigation of derivatives of the two type molecules, such as nitro and bromine substitution products. The study of the behavior of the isomeric chlorides with aniline, urea, thiourea and other substances yielded important results.

The problems that arose covered a wide field and touched many of the more important and fundamental concepts of organic chemistry of that day. Remsen's contributions to the science resulting from his thorough investigations in this field should be classed with the work of the leading chemists of his time.

In 1884 Remsen published his first paper on the sulfonephthaleins. The similarity in structure between phthalic anhydride and the anhydride of *o*-sulfobenzoic acid led him to study the condensation of the latter with phenols. Difficulties were encountered in the purification of the products of the reaction but the work yielded finally a series of compounds analogous in structure to the well-known phthaleins. Compounds of this class have found use as indicators.

Remsen's chief contribution to inorganic chemistry was his systematization of the facts relating to double halides. When he was preparing his textbook of inorganic chemistry he made a detailed study of compounds of this class with the expectation of finding some general principle underlying their molecular composition. As a result of his study of the literature he formulated his conclusions in the following law: *When the halide of any element combines with the halide of an alkali metal to form a double salt, the number of moles of the alkali salt which are added to one mole of the other halide is never greater and is generally less than the number of halogen atoms in the latter.* Seeking a reason for this fact he was led to propose a possible structure for double halides. He noted the analogy between the composition of oxygen acids and double halides and offered the view that in the latter two halogen atoms in combination function as a single oxygen atom. For example, the

compounds having the formulas K_2PbO_2 and K_2PbI_4 could be considered as constituted as follows:



Remsen pointed out many such relationships. He did not claim originality in his point of view and states clearly that many ideas in his paper had been previously suggested by others. He emphasized, however, the advisability of the correlation of the facts and suggestions scattered through the literature of double halides. His paper and the subsequent work that it inspired in his and other laboratories centered the attention of chemists on this part of inorganic chemistry.

Remsen was intensely interested in the subject. He used to draw from it a lesson for his students. In considering the subject he would write the formula for a double salt as usually given, for example, $PtCl_4 \cdot 2KCl$ and pointing to the period in the formula would remark with a twinkle in his eye: "That period has for many years been a full stop to thought. Don't let such devices keep you from trying to find out what lies behind them."

Remsen found in the literature descriptions of a number of compounds the composition of which did not accord with his law. He immediately undertook their investigation and extended the work to the preparation of double halides that had not been described. As a result, a number of those reported as capable of formation were found not to be definite compounds but mixtures. Out of over 400 salts, only three or four did not fit his generalization. To explain the possibility of their

formation the concept of an atomic complex of the type $-Cl \begin{array}{c} /Cl- \\ | \\ \backslash Cl- \end{array}$ was introduced.

The structure of the double halides proposed by Remsen has not been generally accepted. Shortly after its proposal Werner, in 1893, proposed his theory of coordination and much later G. N. Lewis, Sidgwick and others have given electronic

interpretations of the relations involved. Remsen's accurate experimental work in this field is, however, of permanent value.

In one of Remsen's earlier papers (1884) are reported the results of the study of the effect of structure on the reactivity of the bromine atom in the members of a series of alkyl bromides. He first studied the replacement of the halogen when the compounds were reduced by means of zinc and an acid. The normal bromides were found to decrease in stability with increasing molecular weight. Isopropyl bromide was much less stable than the normal bromide. When the reduction took place in alkaline solution the order was reversed and isopropyl bromide was much more stable than the normal bromide. With ammonia and with alcoholic sodium hydroxide normal propyl bromide was the more reactive of the two. With silver salts, however, isopropyl bromide reacted faster than did normal propyl bromide. The pressing problems that were arising in his other work led Remsen to forsake this field of research. It was only toward the close of his activity in the laboratory that he returned to the study of a problem that had always held his interest. In a paper published in 1899 he points out that he had frequently observed that the rates at which compounds of a certain type enter into reaction with the same reagent are dependent on the nature and position of the substituents present. He had observed this particularly in the case of acid amides. Accordingly a research was planned to study the relationship more fully and accurately. He notes that the results obtained were "most interesting." The work was carried out from the modern point of view. The effects of change in temperature and of the concentration of acid or base used were studied. Velocity constants were determined for the hydrolysis of a number of amides containing a variety of substituents in the ortho, meta and para positions. This pioneer work is becoming more significant in the light of similar studies carried out today.

In the beginning of Remsen's work he published several papers of interest on a variety of subjects, but as the problems developed he was forced to center his attention on the more important ones. Two of his early communications describe his study of

the influence of a strong magnetic field on chemical action. He discovered that iron under these conditions is more feebly acted upon by hydrochloric acid at the poles and that copper deposits on the iron in rings perpendicular to the field of force of the magnet. The results appeared to be of sufficient interest to Mendelejeff to warrant calling attention to them in his textbook of chemistry. Remsen showed in another paper that finely divided iron absorbs nitrogen.

When Remsen went to Johns Hopkins University in 1876, there was no satisfactory medium in America for the publication of an account of his researches. A few of his articles were published in *The American Journal of Science*, but Professor Dana, the editor of that journal, soon decided that researches in organic chemistry did not furnish material of sufficient interest to his readers and advised publication abroad. Professor Remsen was not satisfied with this and, with the aid of other chemists, he established *The American Chemical Journal*. With far-sighted vision, he made this a medium of publication for American chemists and not an organ of the Johns Hopkins University. For thirty-five years this journal was a very important agency for the promotion of genuine chemical work. It was the first American journal in this field which secured widespread recognition abroad and it would be difficult to overestimate its value in stimulating chemical work and in placing Americans in their rightful place among the chemists of the world. At the close of the fiftieth volume President Remsen decided that publication in America would be better served by incorporation of *The American Chemical Journal* with *The Journal of the American Chemical Society*. This was done and the latter journal carried on its title page a record of the consolidation of the two journals, until the time of his death.

In the last number of *The American Chemical Journal* Remsen said: "The American Chemical Society has grown to great importance and is amply prepared to provide for the publication of all articles on chemical subjects likely to be prepared in this country. . . . Taking everything into consideration it now seems best to the editor to place the control of his journal in

the hands of the society. It is needless for him to say that after 35 years of editorial work he does not now withdraw from it without a feeling of deep regret. His earnest hope is that the step may prove wise."

When the University of Chicago was organized Professor Remsen helped in the preparation of the plans for the Kent Chemical Laboratory and President Harper made him a very flattering offer to induce him to accept the position of Head of the Department of Chemistry. The students and all his friends in Baltimore were very happy when he finally decided to remain at Johns Hopkins.

Remsen served as acting President of Johns Hopkins University while President Gilman was in Europe in 1889-90. When President Gilman retired in 1901 he was chosen as Gilman's successor. The resources of the university had been depleted by the depreciation of some of its securities and the period of his administration was a difficult one. In spite of this, the university continued a steady and satisfactory development. The school of engineering was founded and the cramped quarters in the heart of the city were exchanged for the magnificent campus which the university now occupies on the outskirts of Baltimore.

Remsen's motto as President was, "Every man does his best work when he is allowed to do it in his own way."

Professor W. H. Howell says of him, "The many criticisms that in recent years have been directed toward this (the president's) office in our American institutions are certainly not applicable to him. He never abused the power placed in his hands, there has been no autocratic interference with the autonomy of the individual departments, and above all there has been no suspicion of indirection in his dealings with his staff. . . . We have been very contented, happy and prosperous under his administration."

In 1881 Boston had trouble with her water supply and Remsen was called on for his advice. During the same year he undertook an investigation of the organic matter in the air and

a study of the impurities in the air of rooms heated by hot air furnaces and by stoves, for the National Board of health.

In 1909 he was appointed by President Roosevelt as Chairman of a Board commissioned to study problems connected with the administration of the pure food law. The other members of the Board were R. H. Chittenden, J. H. Long, G. A. Herter and A. E. Taylor. After the death of Dr. Herter he was succeeded by Theobald Smith. The publicity and the political and commercial influences connected with the work of this Board were very distasteful to President Remsen.

After the great fire in Baltimore, in 1904, President Remsen was the most important member of a Commission which prepared the plans and had charge of the construction of a system of sewers and of sewerage disposal for the rebuilt city. It is doubtful if the large sums of money required were more honestly expended in any other large American city.

President Remsen retired in 1913. After that he spent his time in travel, in revising his books, in work for the government as chairman of the Referee Board, and in consulting work for one of our largest industrial corporations. He died at Carmel, California, in 1927, at the age of eighty-one.

Remsen was the recipient of many honors. He was chosen a member of the National Academy of Sciences in 1882. He was President of the Academy, 1907-13. He was President of the American Chemical Society in 1902; of the American Association for the Advancement of Science in 1903; of the Society for Chemical Industry in 1910. He was an honorary member of the Société Chimique de France; of the Pharmaceutical Society of Great Britain; of the American Chemical Society; and honorary fellow of the Chemical Society (London). He was medalist of the Society of Chemical Industry in 1904, and received the Willard Gibbs medal in 1914 and the Priestly medal in 1923. He was awarded many honorary degrees: LL.D., Columbia University, 1893; Princeton University, 1896; Yale University, 1901; University of Toronto, 1902; Harvard University, 1909; Pennsylvania College, 1910; University of Pittsburgh, 1915; D. C. L. University of the South, 1907.

Remsen was married in 1875 to Elizabeth H. Mallory, a daughter of a New York merchant who with his family spent his summers in Williamstown. He left two sons, Ira M. Remsen, an artist, who died in 1928, and Charles M. Remsen, a surgeon, who lives at 515 Park Avenue, New York City.

In his boyhood Remsen was reared in a very strict, religious atmosphere and he retained a simple religious faith throughout his life. In his address "On the Life History of a Doctrine," delivered as president of the American Chemical Society, after pointing out that "faith is called for at every turn in scientific matters as well as spiritual," he said, "It would be as illogical to give them (atoms) up as it is, in my opinion, to deny the existence of a power in the universe infinitely greater than any of the manifestations familiar to us; infinitely greater than man; a power that 'passeth all understanding.'"

Ira Remsen's ashes rest in a beautiful new laboratory dedicated to his memory and named Remsen Hall. The men who were reared as chemists in his painstaking hands have established a fund in his honor to be used in furthering research in chemistry at the University.

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INVESTIGATIONS CARRIED OUT BY IRA REMSEN OR
UNDER HIS DIRECTION CHRONOLOGICALLY ARRANGED

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J. C. W. FRAZER

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- L. H. Reyerson and L. E. Swearingen. The catalytic activity of metalized silica gels. V.—The oxidation of ethylene.
- Eugene C. Bingham and Theodore R. Thompson. The fluidity of mercury.
- Frederick H. Getman. The color of iodine solutions.
- Martin Kilpatrick, Jr. Catalysis in the hydration of acetic anhydride.
- William A. Noyes. The interaction between nitrogen trichloride and nitric oxide. Reactions of compounds with odd electrons.
- Charles Snowden Piggot. Radium and geology.
- C. N. Meyers and S. F. Acree. III.—The reversible addition of ethyl alcohol to para-bromobenzonitrile catalyzed by sodium, potassium and lithiummethyllates.
- E. P. Wightman. The fogging by acids and oxidizing agents and the intensification of the photographic latent image.
- Laurence L. Quill and Pierce W. Selwood with B. S. Hopkins. Observations on the rare earths. XXX.—Studies in the absorption spectra.
- Roger K. Taylor. Three laboratory devices: a vacuum stirrer, a pressure alternator and a gage for measuring low pressures of permanent gases in condensable vapors.
- Frederick K. Bell. The infra-red absorption spectra of organic carbonates.
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- G. H. Latham. The thickness of absorbed vapor films. II.
- Donald H. Andrews and Ellis Haworth. An application of the rule of Dulong and Petit to molecules.
- L. McMaster and A. C. McGill. Some properties and transformations of ortho-dichloro-4-nitrobenzene.

- James F. Norris and Spencer Ward Prentiss. The reactivity of atoms and groups in organic compounds. VII. The influence of certain solvents on reaction velocity—adjuvance.
- F. O. Rice and Joseph J. Sullivan. Catalytic studies on acetoacetic ester.
- C. A. Jacobson and H. A. H. Pray. Fluosilicates of some organic bases.
- A. W. Dearing and E. Emmet Reid. Alkyl orthosilicates.
- Francis H. Case and E. Emmet Reid. Some 1, 2-dialkyl cyclopentane derivatives.
- Lee R. Herndon and E. Emmet Reid. The decomposition of organic compounds at high temperatures and pressures.
- Austin M. Patterson. The nomenclature of parent ring systems.
- Joseph S. Chamberlain and Malcom F. Dull. The preparation of compounds of malachite green and of phenolphthalein by means of the Grignard reaction.
- E. P. Kohler and N. K. Richtmyer. Pseudo-bases and their salts in the isoxazole series. Second paper.

IMPRESSIONS OF IRA REMSEN ²

BY E. T. ALLEN, PH.D., 1892

Of my first meeting with Doctor Remsen I have now no recollection, but I know it occurred early in October just forty years ago this year (1927). It was probably on the same day that I first saw President Gilman. He personally received my credentials for matriculation, as the custom of the University then was, and I recall his tall, fine figure, animated expression, and vivacious manner distinctly.

Johns Hopkins formed a marked contrast to the quiet New England college where I had just graduated, indeed in many ways it was unique among educational institutions. Materially speaking, the University of that day was a heterogeneous aggregation of three fair-sized laboratories, an administration building which did miscellaneous duty, and a number of old brick dwellings which housed the less popular departments—all located in a busy, noisy, dusty part of old Baltimore. There was little open space or green-sward about any of the buildings and no trees. Street cars passed each side of the group. The sound of the city's traffic penetrated the walls. Heavy trucks, lumbering over the uneven cobblestones, rattled the windows and set the balances a-tremble, while a fire-engine stationed close by rushed forth at intervals to loud alarms, considerably distracting the attention of the students. "Is this the Johns Hopkins University the papers are braggin' so much about?" asked a foreigner with a strong Irish accent whom I once met on Little Ross Street. "Why, Trinity College, Dublin, would turn this into an academy."

Even on paper it was not much of a *university* as the word was generally understood. There were no departments of law, medicine, or theology. Some of the sciences were hardly recognized at all. Many languages, and I know not what other subjects taught in other schools, were omitted from the curriculum and in some instances a single instructor had to stretch his activities to cover more than one field.

² From Johns Hopkins Alumni Magazine, March, 1928.

An examination of the records of the Johns Hopkins of that time would show that there were then close to five hundred students about equally divided between graduates and undergraduates—the influence of the graduates decidedly dominating. I have been connected with half a dozen collegiate institutions as student or teacher but nowhere have I ever seen a student body so thoroughly absorbed in intellectual work. This exceptional group of young men—at any rate the graduates—were drawn to Johns Hopkins by the fame of its faculty and perhaps even more by the belief that it offered unusual opportunities for original scholarship. It had been widely heralded in the press as a new type of school in America, a research institution like the great European universities.

The Johns Hopkins of that period was not well housed, still the buildings were better adapted to work than one might suppose and the equipment was excellent. The real strength of the University, however, lay in its faculty. It was President Gilman's statesman-like plan, now known to all educators, to subordinate everything else to the selection of an able faculty, and he searched Europe and America with care, under the best advice, offering high salaries and other attractive inducements to get the best men available to head his departments. Of this group Remsen was an outstanding figure.

My earliest recollections of Remsen are indissolubly connected with his lecture-room. I retain a very distinct impression of the contrast between my previous vague and faulty conceptions of chemistry and the new world that now unfolded under his skillful touch. There was a rapid expansion of my mental horizon, a strong sense of reality gained in the materials of the subject, and a clear view of their significant relations. The whole field took on a different aspect in the light of his clear exposition.

Professor Remsen at that time was forty-one years of age, a man not above medium height, of rather stocky, slightly rotund figure. His head was round and bald, his face full and somewhat florid, his eyes decidedly blue. He wore a short, well-trimmed beard and used glasses, I think, only occasionally.

He was always well groomed, dressed neatly and in the best of taste, and his hands were immaculately kept. Not a very striking exterior—which probably explains why I do not remember my first sight of him. You might have taken him for a prosperous medical man. He had an excellent voice, clear and strong, of baritone quality, his enunciation was good without being over-precise, and he always made himself easily heard in any part of a hall or lecture-room. In manner he was live and forceful without self-consciousness and without the eccentricities which distinguish many academic persons. He did have one mannerism which was characteristic. He would sometimes pause in his speaking, cast down his eyes for a moment as if in reflection, and draw down the corners of his mouth into a peculiarly smug expression which was rather amusing.

He had already gained a high reputation as a lecturer. As such he is remembered today by hundreds of his old students, and in that rôle he will probably be longest remembered. His style may be characterized in two words—simplicity and clearness. Nobody ever understood the beginner better than Remsen and he seemed to have the beginner almost constantly in mind. Realizing how prone mankind is to mask ideas with words, he thought out his ideas with great care and expressed them in terms ruthlessly stripped of all pedantry, avoiding at the same time any suggestion of crudity. "Much of our university teaching is over the heads of the students, *unquestionably*," he used to say. "How would you teach the law of simple proportions when the number of exceptions is greater than the typical cases?" asked a student and teacher. "I wouldn't say anything about that," was the sensible reply. He used few notes and probably rarely or never wrote down a lecture as Williams, professor of Geology and one of his few rivals as a lecturer, used to do. His diction was therefore less literary, more direct. In illustrating his lectures Remsen intentionally avoided elaborate experiments such as frequently fail to work or experiments whose meaning is not readily grasped. Like all good teachers of science, he relied strongly on sight memory, showing in the lecture-room large numbers of specimens of raw materials and manufactured and laboratory products.

Perhaps all this sounds rather trite. Do not all good teachers, one may say, strive for clearness and simplicity? Remsen's distinction lay in the degree he succeeded in following out these principles and in the indescribable way he had of giving reality to his subject-matter and of arresting and holding the attention of his listeners.

His lectures abounded in allusions to hygiene, sanitation, and industrial matters of public interest. Outside his own subject it was the biological and medical sciences with which he was most familiar, but it may be unhesitatingly affirmed that there was never any insinuation that other fields were of lesser importance. His vision was bounded by a broad horizon rather than the narrow vista which too often restricts the outlook of the scientist.

Remsen succeeded remarkably with beginners but I think his success with more advanced students was not quite so great. I remember a certain feeling of disappointment which I felt in listening to a course of his lectures in organic chemistry for the older students which was offered only once in two or three years. There was new material, of course, but few if any new principles were presented, few new vistas opened, few important relations, scientific, industrial, or economic expounded. It all seemed like the earlier course somewhat "revised and enlarged."

I believe that in the late eighties "journal meetings" were an innovation in America. They are probably common now and I hardly need stop to say how much the student gained from them in the knowledge of chemical literature which was so much emphasized at Johns Hopkins. This, of course, was a German institution imported. In fact the Chemical Department was quite largely an adoption of German ideas and German methods. All our instructors with the exception of one laboratory assistant were German trained, as the best chemists of their generation almost always were. Remsen used to say he grasped the thought of a chemical treatise more readily in German than he did in English as most of his studies had been pursued in the former. Scientific research was not yet thoroughly at home in America and I think I vaguely realized the dependence on German authority, yet I am bound to admit there was never any sense of

unfairness toward the best that was being done in other countries. In these meetings generally Remsen was the responsible man, presiding and carefully following everything. His own reports, like his lectures, were always well prepared and presented. This was not true of all the other instructors and I remember one occasion when a very obvious lack of preparation led to a humiliating rebuke from Remsen.

In the final examinations leading to a degree at the University both written and oral tests were required. In the oral tests the examiners allowed themselves almost any latitude. It was said that one of the candidates in my time was asked some question by Remsen which he could not answer. When the examination was over he approached the professor and said he would like to know the answer to that question. Remsen frankly replied that *he* did not know. The astonished student then said, "Would you mind telling me, Professor, why you asked that question?" "Oh," said he, "I have here a good many bright young men who read widely, and I thought you might have found the answer."

Every day as a rule, following I believe a German custom, Remsen used to see each one of his research pupils to find out how investigations were progressing, and occasionally he made the rounds of the laboratory to show his interest in the work of the less advanced students. His questions were usually aimed at the larger aspects of the problem in hand and the general avenues of approach rather than at special means. Laboratory methods in themselves apparently had little interest for him. The ease with which a student may lose himself in a maze of laboratory directions was capitally brought out by Remsen in a talk I once heard him give on "teaching." "I go into the laboratory," he said, "and find a student busy about his work. I inquire into his activities, whereupon he goes to his manual, runs his finger down the open page and says, 'I am doing *that* experiment.' When I ask further what it's all about, he replies, 'I was leaving the reading till tonight.'" All this acted out to the life in most entertaining fashion.

Remsen never did any experimental work in my day. I have heard him regret that in consequence of it he felt in some degree

out of touch with the experimental work of his pupils. He "could not find the time," which meant, of course, that he was more interested in his lectures and in his writing. He had his own organic field which he cultivated through his students, and he will be remembered as the discoverer of saccharin. His work on the double halides aroused much interest and led to further work elsewhere. Regarding the latter he remarked that a student came to him one day in some excitement, saying that a *man* had come out with the idea that chlorine may have a valence of three or more. "Well," said Remsen, "what do you think of it?" "Why," said he, "I think it's absurd." "Then," said Remsen, in great glee, "I told him I was the man."

I have heard Remsen tell of the discovery of saccharin but quite without animus toward Fahlberg. The latter had the shrewdness to seize upon and to capitalize the sweetness of saccharin, which possibly Remsen might not have done, and he was, like many another scientist, not generous with the credit. I have heard from Morse what I presume is well known, that a German professor with whom Fahlberg had been associated cabled to Remsen, "I warn you of Fahlberg."

Others know the researches of Remsen better than I and will know better how to appraise them. Here obviously we are concerned with *impressions*, not with judgments, but without any wish to disparage him, it is doubtless true that any of us could point out a number of American contemporaries and many more European ones who would be generally regarded as greater investigators.

Johns Hopkins at that time was thought to be and probably was more thoroughly steeped in the ideals of research than any other institution in America. The ideals were constantly before us and Remsen had in this connection one piece of advice which deserves to be perpetuated. "Begin the investigation of some subject that interests you. Don't wait until you can think of something really great enough for you to do." Despite all this I have thought that Remsen, if he had engaged in research "with his own fair hands," in the words of Hill (of Harvard), would have gained immeasurably as a guide in actual scientific investigation.

He was always quick to head off a student who indulged in reckless speculation. He spoke to me once of the "trouble" he had with a former pupil of whom I knew, trouble in holding him down to actual facts. That, I should say, was Remsen's habitual attitude. But, irreproachable as this attitude may be, I think his students got little encouragement even in legitimate speculation. A man not naturally much inclined to speculation himself, I think he rather discouraged theorizing as an exercise dangerous perhaps to all but the greatest minds.

Hill once expressed to me regret that Remsen should expend so much of his energy in writing text-books. I used several of his books in a short period of teaching years ago but got no great impression from any of them except the *Organic Chemistry*. That, I believe, was a unique achievement judging by the interest it arouses in the student and its wide adoption in the schools. His books are marked by clearness and conciseness. "The style is simple, sometimes purely so," said a critic (I think Thorpe) of one of them. This of course was merely a smart shot but there was just enough truth in it to make a hit. I have examined none of the text-books for years and do not know how widely they are used today, but it is my impression that through them Remsen accomplished his purpose in reaching the wider audience which he sought as an educator. But, with the possible exception of the *Organic Chemistry*, none of them is distinguished by a new view-point, a new classification, or an original interpretation of a large body of facts brought to light by the author, such as mark the great text-book.

Remsen was very proud to be the founder of the *American Chemical Journal*. It was for many years foremost in its field and it is a pity it could not have been perpetuated. An article was once sent to the *Journal* for publication by one of the Harvard faculty in which the word "benzol" was repeatedly used. Remsen was much disturbed by it. He insisted on "benzene." This may seem rather pedantic in the face of an old and common usage, but I think he was wholly concerned here with consistency because it was in the interest of clarity.

In the late eighties the *Journal of the American Chemical Society* was a very weak organ. There was little of it and what

there was, was of poor quality. Remsen in reviewing one of its issues in the Journal Meeting, pointed out at length its obvious defects. "And *that*," said he, with some vehemence, slapping down the journal on the table, "*that* purports to be the official organ of *American* chemistry."

Remsen's attitude toward American chemical industry in that day was unfortunate. The intimate association which existed between the German universities and German industry and the mutual advantages that grew out of the association was a theme not infrequently touched upon in his lectures. Yet there was little attempt made by him to direct any of his students into industry; all his encouragement appeared to steer them into teaching. I suspect he may have had some unfortunate experiences with industrialists. He used to speak ironically of those who were boastful about operations "on the large scale" and I have heard him speak of being refused an entry into some of the Baltimore works. I dare say there may have been as much prejudice on the other side toward the schools, but if he had been a more tactful man prejudices might have been swept away and new opportunities for students might have been opened as well as new avenues of influence for a man so really practical as himself.

Remsen was sane and practical, eminently reasonable, never off on a tangent, direct and forceful, and should have made a great executive had he been possessed of sufficient tact. Humor he had in good measure and he knew how to make it tell in his teaching. I wonder if everybody has heard him relate how Wöhler (a teacher of Remsen) once making the rounds of the laboratory came upon a student leaning on his elbow and gazing intently at a solution on his table, apparently lost in thought. "What are you doing?" said Wöhler. "I'm crystallizing," said the pupil impressively. "Well, don't move," was the reply.

One of Remsen's best qualities was the power of balanced judgment which he possessed in unusual measure. He had the faculty of discussing a subject dispassionately, stating the facts pro and con and leaving to the listener often the right of decision. Oleomargarine, then of considerable public interest, was one subject which I remember as well summed up. Touching

on the effect of alcoholic beverages he decried the misleading methods of some "temperance" lecturers of the day. "Are we to suppose," he exclaimed, referring to an experiment in which alcohol was poured on white of egg, "Are we to suppose that everytime we take a drink of beer we coagulate our albumen?" "But," he continued, referring I think to the 'liquor of the good old times,' "we must remember that the real poison in liquor is alcohol."

Illustrating the same characteristic Remsen related with great satisfaction how an eminent mathematician (I think it was Sylvester) had once attended a course of his lectures in organic chemistry. The mathematician was interested in the number of isomers which an organic compound may have. Remsen had evidently proceeded in his usual cautious manner, stating the evidence and showing what the theory predicted without actually committing himself to it. The evidence had been piling up when one day after the lecture Sylvester came to him and said with ardent conviction, "Aren't you *satisfied* this theory is true? I am."

Everyone who knew Remsen was aware that he was somewhat irritable. He worked hard, taking the bulk of the teaching of chemistry on his own shoulders, and though he was rarely incapacitated I presume he was inclined to overdo.

On one of his visits he came to the "quantitative laboratory" where I happened to be working. A strong odor of ammonia pervaded the air. "What's all this? Who's responsible?" said Remsen in his quick way. Some student mentioned the name of Dr. B. who was at work in the basement. He was a man much older than the average, a real professor in a "Western" college and not much in awe of authority. Professor Remsen, sighting Dr. B. at that moment, exclaimed with great vehemence, "This is unbearable, Dr. B., unbearable!" "Well, what do you think of me?" said Dr. B. "I have to *stay* in it." "I don't care anything about that, I don't care anything about that," was the reply.

One day I happened to be talking with Remsen in his office, by appointment in all probability, when there came a knock at the door. Remsen paid no attention to it, but the knocker,

probably thinking he had been invited to enter, opened the door, and stepped in. He came up to Remsen hat in hand, a most awkward figure, a veritable Guy of Gisborne, as green of appearance as can well be imagined. Remsen's sense of dignity was instantly affronted by this unwarranted intrusion on his privacy. "My name's (we will say) Dubb," was the embarrassed remark. "Well, what of it?" snapped Remsen. "I wanted—" "Well, close the door, please, till I am at leisure." The poor fellow closed it, remaining *inside*. "Outside, please, outside," said Remsen.

My own relations with Professor Remsen were uniformly pleasant and that I believe was the general experience of his students. They were by no means as close as my associations with the genial Morse, a matter partly accounted for by the direction of my studies.

The handwriting may be an indifferent index to a person's character but it is a rather interesting fact that Remsen wrote an admirable hand. I still recall it after the lapse of years. The letters were even, the lines well spaced, the whole refined and conspicuously clear—such a hand as anyone would like to possess.

The extent of Remsen's influence and fame, especially in the educational world, is undoubtedly great. He had not, however, the picturesque exterior, the magnetic personality, the dazzling gifts which perhaps sometimes beguile us into overestimating a man. To some future biographer who has dispassionately studied his subject and mastered his materials must be left the task of measuring the breadth and the depth of Remsen's inspiration in the lives of men and its motive power in the development of chemistry. That he was a remarkable teacher would be the unanimous verdict of all his pupils.

I still visualize Remsen best in his lecture-room. I hardly need close my eyes now to call up my earliest impression of him and the strongest. There, behind his table in the old laboratory on Little Ross Street, he stands as he was in his early forties, a forceful if not magnetic figure, commanding the respect if not fully winning the affection of his pupils, doing the work which he loved best and fulfilling the mission for which Nature clearly intended him.

ECHOES FROM THE REMSEN MEMORIAL MEETING³

MR. WHITRIDGE'S ADDRESS

When the President of the Johns Hopkins University asked me to tell you of a contact I had with Dr. Remsen, I gladly consented, for I realized at once that it would be a theme of great joy to me to tell you, his friends, of my association with him, possibly in a way few knew him. I have since recognized that it would need an abler pen than mine, and a more learned and eloquent tongue to do justice to his memory.

Excepting two others, I know of no one person who has influenced me more than Dr. Remsen. He aroused in me the principle, that whatever a man's limitations may be, some of the talents God has given him should, in part at least, be used for the good of others, and especially for his native city, and not entirely for his own personal interests. To you his former colleagues, now so few in number, to you his former students, whom he taught and inspired, and to you his friends, gathered here to-day to honor the memory of a man you loved and respected, I shall speak of a characteristic, probably little known to the scientific world in which he lived, and tell you of a point of view I got of his make up and character during a period lasting nearly eleven years.

Many of you may recall, that after the great fire of 1904, Baltimore awoke from a semi-comatose state of mind in respect to public improvements. Up to this time we had been a big village. Now the citizens of Baltimore demanded that the Burnt District should be rebuilt in a modern and up-to-date way, and above all, that an adequate and proper system of sewerage should be constructed. Strange to say, until after the great fire, Baltimore had depended entirely on a system of cess-pools and a number of privately owned sewers draining into Jones Falls, or the Upper Basin. There was only one other large city with such an antiquated and unsanitary method for the disposal of sewerage, viz., Cairo, Egypt. For this improvement large sums of money had to be spent, and the Public and Press

³ From Johns Hopkins Alumni Magazine, November, 1928.

demanding that the work be done honestly, efficiently, and without graft, and that the members of the Commission should be men of integrity and uprightness.

It was my privilege to serve on that Commission from the beginning of the work, in June, 1905, until we voluntarily resigned as a body, in January, 1916. The outstanding member of the Commission through these eleven years was Dr. Remsen, the then President of Johns Hopkins University. My other fellow-members were honorable and capable men.

We built a disposal plant on Back River practically automatic in its working; we sewered hundreds of miles of streets, connected to our system thousands of houses and factories, and asked for and spent over twenty-three million dollars. I think our work was well done, for I am told, that even to-day, the Baltimore system is as modern and up-to-date as any, and would cost to duplicate sixty-five millions.

When I was asked by the Mayor to go on this Commission I hesitated, and requested time to consider. Naturally, I consulted my father. At first he advised me to decline, with the remark, that he would regret to see any son of his mixed up in a political job. I remember telling him I thought politics could be kept out, and stated that Dr. Remsen would be a member. Quick as a flash, he said: "If Remsen will be on that Commission, accept by all means, for he will keep things straight."

Business men often say that scientific and learned men are lacking in hard, common sense. With Dr. Remsen this was not the case. He grasped a business matter quickly, and demanded and expected order and system to prevail. I recall, at the very outset, I asked him how we should start our work. Should we visit other cities and see how they were handling such problems?—"No," he said, "neither you nor I know anything about handling the situation ahead of us. As a chemist, I know something about the composition of sewage, but I know nothing as to the proper method of its disposal. The right way to handle this problem is to get the best experts in the country to tell us what to do." Through his contact with the leading technical educators, and especially through the help and assistance of his

friend, President Pritchett, of the Massachusetts Institute of Technology, now head of the Carnegie Foundation, we had for our advisers the three leading sanitary engineers of the United States, Messrs. Herring of New York, Stearns of Boston, and Gray of Providence, and later on, when in a very animated fight with a City official—a man of little experience and education—William Barclay Parsons, who helped to plan the Panama Canal, acted as a consulting engineer to us. In all the preliminaries to get these consulting engineers, Dr. Remsen took a leading part. With pride I can say, that due to Dr. Remsen's assistance, the total amount of their fees was less than \$25,000, a sum well spent when millions were to be expended, and over a period of eleven years.

For eleven years I was chairman of the Committee on Accounts, and all bills and estimates had to be passed on by two members. I realized from the beginning the necessity of having our books properly opened and audited by a reputable audit company, and requested authority to employ such a concern. Our then Mayor hesitated and suggested an accountant employed in the City Hall. For obvious reasons this would not do, although the man was capable. I personally went to Dr. Remsen and told him how necessary it was to have a firm in whom the public would have confidence. Through his help and tactfulness with the Mayor, I had my way, and we employed a firm of reputable accountants; our books were opened properly, and at the end of each year audited. We were the first City department in Baltimore to inaugurate yearly audits. The cost was very little, but the satisfaction to all our members was very great, in knowing that our accounts were correct and in order.

Again, I recall a fierce and protracted dispute with the City Council regarding our plans. We had anticipated Baltimore's needs up to one million people in the large out-fall tunnel or sewer to Back River. Egged on by some of the political contractors, who had received none of our contracts, for weeks communications passed backward and forward between our office and the City Council, with queries from them and answers from us. One Thursday afternoon came the communication, that on Monday, by 5.00 p.m. the Council must have our reply.

To meet these demands from the Honorable City Council, on a number of occasions we spent a large part of Sunday in our office, and Dr. Remsen, busy man as he was, always was on hand. We finally won out, and satisfied the City Fathers that we knew our business and for the next eight years peace and quiet prevailed, and we were left alone.

In one respect we were a very remarkable body, for during the eleven years only three times, when we came to a final vote, was there a member or members dissenting. When it was apparent after a discussion, that there would be a divided vote, the usual method was to form a Special Committee, and place the dissenting member or members on it. The matter was carefully gone over in these Committees, and a report drawn up. This report was unanimously accepted by the Commission, and Dr. Remsen was generally chairman of these Committees. So we worked in harmony, and with respect for each other, and there was no ruction nor friction.

As to the man himself, I never saw him perturbed or upset by pressure of work. He was always genial and kindly, and while we knew that by education and ability he was the leading member of the Commission, he never tried to force through his views, always listening with deference and interest to those of others. There was never any petulance or arrogance about him, even if he held the degree of LL.D. from our leading universities. His manner toward his fellows was always kindly and approachable, and do you wonder we respected him and felt we always had a sheet anchor to windward?

In my eleven years' association with him only once did I see him depressed. It was after an operation at the Hopkins Hospital, shortly after he resigned as President of the University. He came to a stated meeting of our Commission. I asked him how he felt, and his reply was, that he felt better, but intended to resign from the Commission, as his days of usefulness were over, and remarked: "I am a has been." I remember telling him what he had meant to us, and how he had started us on the right track in our work, and how he had gotten the best men in the country for our consulting engineers. I felt, if he then gave

up his membership on our Board, the future might not be so pleasant, nor our work go on so satisfactorily, as the press respected him and the politicians looked up to him. I saw his face light up after my talk, and he said: "If you really believe I am of some use, I will stay," as he did until we went out of office in 1916.

Again, I remember going to Annapolis with him and some of the members of our Commission. There was an iniquitous bill before the Legislature. "The Professor," as the politicians called him, and Edgar Allan Poe, then City Solicitor, met in conference with some of the sponsors of the bill, and after some time had passed, Dr. Remsen came back to the rest of us, and gleefully remarked, that he and Mr. Poe had made a compromise. That compromise took all the sting out of the bill, for he and Edgar Poe together had "put one over" the politicians, and saved the City thousands of dollars, for the bill, as originally drawn, would have forced us to buy most of the private sewers. The compromise read: "if they were properly built and could be used in our system," and very few were used.

So I might go on from incident to incident, with President Remsen taking a leading part in everything we did, serving more than any other member as the head of committees, and always ready to help.

When we went out of office in 1916 I saw him at intervals, for his work took him to New York and Chicago, and afterwards he lived most of the year in California. Whenever he came to town I tried to see him, and he was always interested to hear about his colleagues, and what our former engineering staff was doing. The last time I saw him was in the fall of 1926. He had returned to Baltimore for the University's Half-Century Exercises and Dinner. His mind was clear, and I was surprised how well he remembered names and incidents.

When I said good-bye to him, in the quizzical way he often had, he shook my hand, and remarked: "I wonder how our Versailles fountains of sewerage are working at Back River, and is that balmy breeze still coming from the sludge digesting tanks? I should like to go there again with you."

I have missed him, and shall miss him, and am glad to tell you, his friends, in this simple way, what I thought of him, and how much I respected, admired, and loved him.

Many years ago Robert Burns wrote some verses about a dear friend; with some slight changes, may I use them in concluding:

An honest man is now at rest,
As e'er God with His image blest.
The friend of Man, the friend of Truth,
The friend of Age, the guide of Youth.
Few hearts like his with virtue warmed,
Few heads with knowledge so informed.
If there is another World he lives in bliss,
If there is none, he made the best of this.

DR. GRIFFIN'S LETTER

It is a profound regret and disappointment to me that I am unable to be present at the meeting in memory of Dr. Ira Remsen. I have been a patient at the Johns Hopkins Hospital for some weeks past, and have recently come to New York to stay for the present with my son.

My acquaintance with Ira Remsen began in July, 1872. After a residence of upwards of seven years in Germany as a student, he had returned to the United States to enter upon what was to be such a distinguished career. One can imagine the change from so many years of student life in Germany to a professorship in a New England country college,—Williams. This date makes Dr. Remsen, with only one or two exceptions, the oldest of my friends.

Nothing could have been more felicitous than the relations which he established in his new home and in his new duties. He at once showed himself a natural teacher, sympathetic and inspiring.

The chair which Dr. Remsen filled at Williams was a more comprehensive one than would now be considered suitable. He was professor of Physics and of Chemistry, and his predecessor, who was called to the chair of Physics at Yale, had emphasized the subject which was the less congenial of the two to Dr.

Remsen. He was expected to give physics rather than chemistry the more important place; but, if this did not agree with his own wishes, he disregarded them, and there was no one in the staff of professors who was more acceptable as a teacher, as an associate, and as a friend. The only drawback of which he ever complained was lack of time for original research.

When, after four years of most acceptable service, Dr. Remsen was called to the Johns Hopkins University, all of his colleagues recognized the fitness of this promotion.

It is quite unnecessary for me to speak of Dr. Remsen's long and distinguished career at the Johns Hopkins University,—as head of the Department of Chemistry, as the teacher for many years of an undergraduate class, as director of the advanced work of graduate students, as author and editor, and finally as President of the University. The many public duties which he assumed, the wide recognition which he received, both in this country and abroad, his extensive acquaintance among scientific men throughout the world,—all this, which is known to others as well as to me, I will leave unmentioned. If I were to attempt to express my sense of personal obligation to this dear and honored friend, my word would pass the limits which the proprieties of this occasion impose.

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

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VOLUME XIV—EIGHTH MEMOIR

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OF

THOMAS BURR OSBORNE

1859-1929

BY

HUBERT BRADFORD VICKERY

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1931

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Thomas B. Osborne.

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A scientific investigation that is continued without interruption or diversion from the main theme for forty years can be conducted only by one who has to an unusual degree the qualities of pertinacity and patience combined with a love of science and of work for its own sake. Few of those who possess such characteristics are so inspired in their initial choice of a problem or so richly rewarded by its outcome as was Thomas Burr Osborne. His death on January 29th, 1929, removed one of the most distinguished pioneers of American biochemistry, a man whose name will always be linked with the subject he made peculiarly his own and upon which he firmly imprinted his forceful personality.

Osborne's life was one of whole-souled devotion to a single purpose, the understanding of the relationships of proteins to each other and to the animal world. He began his researches upon the vegetable proteins in 1888, at a time when little attention was being paid to these substances either by physiologists or chemists. He lived to see the study of proteins become one of the major topics of biochemistry, and to experience the satisfaction of knowing that this broad increase in interest was to no small extent a direct result of his own efforts. The great leaders in this field of scientific endeavor, Fischer, Kossel, and Osborne, have now all passed to their reward leaving a world infinitely the richer for their labors.

Osborne's entire life was spent in New Haven, Connecticut. There he was born on August 5th, 1859, there he received his education and scientific training, and there he did his work. He was seldom absent from his laboratory and his outside interests were few. He cared not at all for organized sports nor for the formal social life of the community, but preferred to take his recreation hunting or fishing or, in later years, in long country walks with a friend. On these occasions his extraordinarily acute powers of observation for the bird life around

him, and his shrewd comments on matters of mutual interest, were never-failing sources of delight to his companion. For many years he was a Director of the Second National Bank of New Haven. At the meetings of the board his keen understanding of business problems and the relentless logic with which he pursued his arguments won him the respect and regard of his fellow members. He led a life of rigid routine, but his adherence to the details of a well-ordered daily life was far from being an index of any lack of imagination. Above all else Osborne lived for the future. His interest in the past was restricted; he desired merely to know what were the ascertained facts, what were the well-supported and most fruitful hypotheses: upon this firm ground he planned his investigations, ever watchful for the obscure signs that point the way to further advances in sound knowledge.

Osborne's heritage is significant. The earliest recorded paternal ancestor in this country was Richard Osborne, born in England in 1612, who settled at Hingham, Massachusetts, in 1635, removed to New Haven in 1639, and later (1650) to Fairfield, Connecticut. His mother was descended from William Blake of Little Baddow, Essex, who came to America in 1630 and settled in Dorchester, Massachusetts; Eli Whitney Blake, the inventor of the stone crusher, was Osborne's grandfather, and Eli Whitney, the inventor of the cotton gin, was a great-great uncle. Through his maternal grandmother he was sixth in descent from the Reverend James Pierpont, a graduate of Harvard in 1681, who became the pastor of the first church in New Haven in 1684 and was one of the prime movers in the formation of Yale College. Osborne was the elder son of Arthur Dimon Osborne and Frances Louisa Blake. From his father, a member of the Connecticut bar, and a banker, he inherited an interest in the daily affairs of men that was developed by informal family discussions into an unusual training in finance. From his mother he inherited the love of trying new things and new ways—the desire to create—that informed his whole life-work.

He received his early education at the Hopkins Grammar School. As a boy he paid little attention to games or other amusements, his chief delight being to ramble through the woods and marshes in pursuit of birds. He rapidly acquired an intimate knowledge of their habits and accumulated a valuable collection of the local species. One of his companions on these expeditions has recently written: "He was intensely interested in all features of Natural History and knew plants and animals quite as well as he did birds, although we devoted more time, perhaps, to collecting birds. I do not know how large a collection of bird skins he had, but it must have been an important one."¹ Many of Osborne's notes and observations of bird life are recorded in the "Review of the Birds of Connecticut" published by C. Hart Merriam in 1877 and some of the skins from his collection are still in existence in the possession of Henry H. Townshend of New Haven.

He was encouraged in scientific activities by his uncle, Eli Whitney Blake, Jr., professor of physics at Brown University. Letters from the uncle refer to the microscopic examination of diatomaceous earth, to the dissection of clams and to other scientific subjects. Perhaps the most entertaining of these letters was written March 5, 1877, and contains a full description of Alexander Graham Bell's then recently invented telephone. Osborne used this information in the preparation of a school essay in which the telephone was described and illustrated by means of a diagram and a working model. He referred, in the essay, to the fact that he had been present when Dr. Bell demonstrated in Providence that music and the human voice could be transmitted over wires from Boston.

Osborne planned to continue his education at Cornell University where facilities for the study of science in the academic course were offered. He was, however, dissuaded and in 1877 took up his studies at Yale, thereby following the traditions of a family of Yale graduates that extended in almost unbroken line from a great-great-great grandfather, Ebenezer Dimon, who was graduated in 1728. The required study of moral philosophy

¹ Personal communication from Dr. Robert T. Morris, New York.

and the classics at Yale held little attraction for him and it is not surprising that his extracurricular activities at this time occupied much of his attention. Nevertheless, he received first colloquy appointments in both junior and senior years and was made a member of Psi Upsilon and of Skull and Bones; rewards that imply conspicuous attainments. He was president of the Yale Society of Natural History during the last three years of his course and surviving copies of the papers read at some of the meetings indicate that this was a group of serious minded young men who welcomed an opportunity for the discussion of scientific topics. It is interesting to note that a number of them were later eminent in various fields and, besides Osborne, at least five² became members of the National Academy.

The most important of Osborne's activities during his college career was entirely extramural. Through a school friend whose father was a prominent miller, he became familiar with a problem that had arisen in the flour milling industry. The existing method for the purification of middlings was in some respects unsatisfactory and millers were eager to secure a cheaper process. The adherence of tobacco ash to a gutta-percha pencil used to ram down the contents of his pipe suggested to Osborne that the difference in the electrical properties of the particles in the ground wheat might be used as the basis of a method of separation. A machine was devised in which the middlings passed under rolls constructed of rubber upon the surfaces of which an electrostatic charge was induced by causing the rolls to rotate in contact with sheep's wool pads. Devices were added to agitate the middlings and to remove and collect the lighter particles of bran which flew up and adhered to the electrified rolls. An American patent No. 224719 was granted to Osborne for this machine on February 17, 1880, and shortly afterwards American and foreign patents covering the original invention and certain improvements were granted to him and to his associates. The electric middlings purifier was used with success

² Edmund B. Wilson, C. Hart Merriam, Joseph P. Iddings, Louis V. Pirsson, and Samuel W. Williston.

for several years in a number of mills but was eventually superseded by other machines.

Osborne took his bachelor's degree in 1881. At that time the most promising career for a man with scientific leanings appeared to be medicine and he therefore enrolled in the medical school where he studied for one year. As this did not prove to his liking, in 1882 he entered the graduate school and took up the study of chemistry under Professor W. G. Mixter; the following year he was made laboratory assistant in analytical chemistry and obtained his first opportunity to embark upon original chemical investigation. He prepared two papers on the analytical separation of zinc which were published under his name alone in 1884, and in 1885 presented a dissertation on "The Quantitative Determination of Niobium," for which he was awarded the doctor's degree. He remained at Yale another year, occupied in research and teaching; in May, 1886, at the invitation of S. W. Johnson, professor of agricultural chemistry in the Sheffield Scientific School at Yale, and director of the Connecticut Agricultural Experiment Station, he became a member of the station scientific staff. In the same year he married Elizabeth Annah Johnson, Professor Johnson's daughter.

During the years which followed Osborne gradually took up the life of a scientific investigator. An influence which had the most far-reaching effects upon his mature life was exerted upon him by his distinguished father-in-law. The two men were united by bonds of the closest nature. Johnson was his mentor and teacher, the director of his early researches and intimate associate; Osborne became Johnson's most eminent pupil, the exemplar of the principles, in the training of investigators in agricultural science, to which Johnson devoted his life. They were in daily contact for nearly thirty years, Johnson the scholar, the master of the literature, the administrator; Osborne the clear and logical thinker, the determined investigator, the careful experimenter.

When Osborne first went to the experiment station laboratory, Johnson had been engaged for some time upon a study of the sugars obtained by the hydrolysis of various plant gums.

He had prepared a number of these substances but had been unable to obtain the services of an analyst trained to conduct combustions for the determination of carbon and hydrogen. The first assignment given the new assistant was the analysis of these preparations. Their nature as pentose sugars was soon established, but, in view of the publication of Kiliani's discovery that arabinose was a five-carbon sugar, and of the extensive investigations of similar substances being undertaken in Germany, Johnson did not publish the results. For several years Osborne was occupied with various problems of agricultural chemistry. He developed the so-called beaker method of mechanical soil analysis and was active in the general analytical work of the station. He also refitted the small home laboratory formerly used by his wife's father and for a number of years thereafter spent many evenings in private commercial work.

The passage of the Hatch Act in 1887 made federal funds available for scientific investigations at state experiment stations. This addition to the very limited resources of the Connecticut station permitted an enlargement of its activities, and one of the first projects undertaken was the study of vegetable proteins.

In order to understand how this came about it is necessary to go back to the year 1853 when Johnson was a student for a few months under Erdmann in Leipzig. One of Erdmann's assistants was Heinrich Ritthausen, a young man who later became interested in vegetable proteins and, in 1872, published a book, *Die Eiweisskörper der Getreidearten, Hülsenfrüchte und Ölsamen*, in which was described an astonishing amount of original research upon the proteins of seeds. Ritthausen's work was continued until the early 90's but was little appreciated either in Germany or in this country. Among the few who grasped its significance was Johnson, who recalled his acquaintance of student days as an earnest and trustworthy investigator, but was convinced that even more thorough studies were necessary before the relationships of the seed proteins to each other and their value in human and in animal nutrition could

be understood. Finally, in 1888, the opportunity to take up this work was presented. In Osborne, Johnson had a highly trained and capable assistant who had already demonstrated his capacity for independent research. The necessary funds had become available. One vacation day in August of that year, while walking up the hill to his summer home at Holderness, New Hampshire, with his son-in-law Johnson proposed that an investigation of the proteins of seeds should be undertaken. The problem was to be Osborne's own and, under the new conditions, he would be able to devote his whole time to it. The younger man eagerly assented and, a few weeks later, began the studies that were to continue throughout his life. Many years before (1845, 1849) J. P. Norton, who had been Johnson's teacher at Yale, had published two papers on the proteins of the oat kernel. This grain had also been investigated by Kreuzler in 1869 but it had not been studied by Ritthausen. In view of this and of its great economic importance the oat kernel was selected for the initial investigation.

Osborne's work on the vegetable proteins falls naturally into three phases. From 1889 to 1901 the chief interest was in the preparation of pure specimens of the proteins of plant seeds. The investigation of the oat kernel (13, 14), published in 1891, was followed by a series of papers in which the proteins from no less than thirty-two³ different seeds were described. At least five of these, flax seed, adzuki bean, cow pea, soy bean and cotton seed were studied for the first time by him. Every seed was found to yield several different proteins and each of these was prepared, where possible, by a number of different methods. All available methods were employed to ensure that the preparations should represent homogeneous material as nearly as possible identical with the protein as it occurred in the seed.

³ Osborne studied in detail the proteins of wheat, rye, barley, maize, oat, rice, kidney bean, adzuki bean, pea, vetch, lentil, horse bean, soy bean, cow pea, yellow lupine seed, blue lupine seed, squash seed, castor bean, hemp seed, flax seed, cotton seed, almond, peach kernel, Brazil nut, hazel nut, English walnut, American black walnut, butternut, sunflower seed, and potato tuber. He also prepared the proteins of the lima bean and coconut but published no detailed paper on them.

The only criterion of the purity and individuality of protein preparations that was known at this time was the ultimate analysis of the material for carbon, hydrogen, nitrogen, and sulphur. That this criterion alone was hopelessly inadequate became apparent later and, since the recognition of this inadequacy was largely due to Osborne and amounts really to a turning-point in the history of protein chemistry, it may be worth while to discuss it more fully.

The early pioneer work of Beccari, Rouelle, Fourcroy, Einhof and Gorham suggested that there are four chief types of vegetable proteins, albumins, which are soluble in water and are coagulated by heat, plant caseins, plant fibrins, and alcohol-soluble proteins that have some properties in common with gelatin. The resemblances between these types of vegetable proteins and the more generally known proteins of blood and of milk gave rise to the idea, clearly expressed by Liebig and upheld by later writers such as Gerhardt and Kolbe, that there are in fact only four kinds of protein in nature, albumins, caseins, fibrins, and gelatins, and that these occur both in plants and in animals. According to this view it was easy to account for the value of vegetable food in the nutrition of animals and, moreover, the notion was philosophically satisfying since it tended towards the simplification of nature. As time went on, certain doubts arose that the relationships between the vegetable and animal proteins were quite as simple as this, but, when Osborne began his studies, most scientists were persuaded that the total number of different proteins in nature was very limited. Moreover, it was almost universally held that proteins from different sources had equal nutritive values. Only a few years earlier Johnson had explicitly stated in an official publication of the station that, according to general belief, vegetable albuminoids do not greatly differ from each other in nutritive effect.

Ritthausen's extensive investigations had been planned to demonstrate that proteins of similar type from different seeds are identical with each other. He had found, however, that there are at least two different kinds of plant fibrins; these he designated as legumin and conglutin, proteins that are classified today

as globulins. The alcohol-soluble proteins also failed to fall neatly into the scheme of general identity and Ritthausen had become convinced that the gliadin of wheat is a mixture of several constituent proteins which differ in their solubility in different concentrations of alcohol. The alcohol-soluble proteins of barley and maize were regarded as mixtures of these same constituents but in different relative proportions.

This was the situation when Osborne's investigations began. As seed after seed was studied and the number of carefully prepared and highly purified proteins increased, it became clear that many of these could no longer be grouped under a common name since they were in fact distinct substances. Specific designations were, therefore, coined and the older names were reserved for those proteins to which they had first been applied. This clarification of the nomenclature has been of immense assistance in bringing a semblance of order into an almost hopelessly confused subject. Gradually the principle was evolved that differences between proteins are more important than similarities and the search for criteria whereby these substances could be differentiated and characterized was pursued with greater and greater energy.

In 1892 Osborne (18) described crystallized globulins obtained from six different seeds and concluded that the globulins from the Brazil nut and the oat are distinctly different substances. The globulins from hemp seed, castor bean, squash seed and flax seed were, however, closely alike. He said, "It is at present impossible to assert that these four globulins are the same, but since differences exist between different preparations of globulin from the same seed as great as those found among the globulins of these different seeds, the writer is disposed to consider these four globulins as identical." Later, in 1894, the amorphous globulins of wheat, maize and cotton seed were shown to have the same ultimate composition as these four crystalline globulins and he wrote (23), "as the properties of the preparations obtained from all these sources are substantially alike, there can be little doubt that one and the same proteid exists in them all. For this body we adopt the name *Edestin*

from the Greek *ἐδεστός* signifying edible, in view of its occurrence in so many important food-stuffs."

In 1896 Osborne pointed out (31) that the globulins from the peach kernel and almond are so closely alike that little doubt of their identity was entertained, further that the globulins of the walnut and the filbert are apparently identical with each other but that these differ from the edestin present in the seven seeds mentioned above. The differences he described had been overlooked by previous investigators and all of these proteins had been classed together as vegetable vitellin, a term which, in view of this newer investigation, Osborne proposed to abandon, since it was "associated with many erroneous statements as to its occurrence, composition, and characters." At this time he had prepared "six perfectly distinct proteids which have been confounded together under the name vitellin or conglutin." These six were *edestin* derived from hemp seed, castor bean, squash, flax and cotton seeds, wheat, rye, barley, maize and the coconut, *amandin* from the peach kernel and almond, *corylin* from the walnut and filbert, *excelsin* from the Brazil nut, *avenalin* from the oat, and *conglutin* from lupine seed.

In 1898 a paper on the pea, lentil, horse bean and vetch (44) described the proteins legumin, vicilin and legumelin. These four seeds were all found to yield preparations of the globulin legumin between which no essential differences were detectable, and the preparations of the albumin-like legumelin from them were also closely similar to each other. A protein that was designated vicilin was found in the first three named. Legumelin appeared to be likewise present in the adzuki and soy beans and in the cow pea.

The papers that describe these investigations are technical and are frequently long; they possess, however, a property that is almost unique in the early protein literature. The operations are so minutely and carefully outlined that it is possible to repeat Osborne's work to its last detail and secure preparations that correspond exactly to those he described.

The chemical and physical properties of many of these proteins were such as clearly to show the advantages for scientific

investigation of the reserve proteins of seeds over the proteins of animal origin. Efforts of others to isolate proteins of definite properties from the complex mixtures in animal tissues had been for the most part unsuccessful, and even as late as 1911 not more than two or three animal proteins had been clearly characterized as definite chemical substances. On the other hand, many seed proteins were early shown by Osborne to be chemically distinct and, furthermore, the preparations were reproducible at any time. He had crystallized a number of the globulins, and the readiness with which this could be done emphasized the fact that these proteins were definite substances entitled to the serious consideration of chemists.

Up to 1899 Osborne had been interested almost wholly in the preparation of proteins from as many different sources as possible. In this year he began to subject his wealth of material to more critical and detailed chemical examination. This marks the beginning of a second phase of his labors, a period in which the properties of the proteins became the matter of chief importance and which culminated in the elaborate amino acid analyses that laid the foundation for the nutrition studies of his later years.

The first paper (47) in which this change in interest is apparent showed that the crystalline protein edestin from hemp seed forms two distinct compounds with hydrochloric acid, that the solubility of edestin in acid increases in direct ratio with the amount of acid present and that this and a number of other crystallized vegetable globulins neutralize definite proportions of acid. In other words, the behavior of these proteins was that to be expected of basic substances of fixed composition. This was one of the ends towards which the careful descriptive studies had been directed, a demonstration that some proteins, at least, have many of the properties of definite chemical individuals. The position here taken was strengthened by other papers in which it was shown that proteins exhibit many evidences of a capacity to undergo electrolytic dissociation and enter into ionic reactions.

These investigations clearly demonstrated the necessity for more complete chemical characterization of the different proteins. The older view, which narrowly limited the total number of vegetable proteins, was manifestly inadequate since a relatively short period of intensive investigation had greatly increased the number of kinds of protein that could be prepared from seeds. But what should be the next step? There was plenty of widely diversified material at hand. How should it be attacked? It is interesting to note how, during the period from 1899 to 1903 Osborne tested first one lead and then another, each time obtaining results important in themselves but not contributing much to the central problem. He worked on egg proteins, on the nucleic acid of wheat embryo and on the sulphur content of proteins. The data of this last paper (54) were used in a discussion of the possible molecular weight of proteins and are still employed for this purpose. Finally the required suggestion was obtained from the extraordinary results of Drechsel, Hedin, and of Kossel and their simple application to the proximate analysis of proteins by Hausmann in Germany. When proteins are boiled with strong acid they are slowly decomposed into relatively simple crystalline substances, the amino acids. At least twenty-one different substances of this type have been secured from proteins and most proteins yield fifteen or more of these. Kossel had attacked the problem from the amino acid point of view and had established the principle that protein analysis could most effectively be accomplished by the quantitative separation and estimation of these simpler derivatives. In particular he had developed a method whereby the three basic amino acids arginine, histidine, and lysine could be determined. With two exceptions all of the known amino acids derived from proteins contain an α -amino group; the three basic amino acids contain additional nitrogen in other parts of the molecule combined in basic structures that have properties different from those of the amino group. It is clear, therefore, that there are several different forms of combination of nitrogen in the protein molecule. This idea had been grasped by Hausmann who in 1899 proposed a simple scheme of protein analysis

whereby the proportion of the total nitrogen that was present in each of three well-marked forms might be determined. Hausmann's method met with considerable adverse criticism but it appealed to Osborne as a means by which additional knowledge of proteins might be readily secured and as a useful preliminary to the far more elaborate analysis of the basic amino acids according to Kossel's procedure. It soon became apparent that, although the method might not yield accurate absolute results, it did yield valuable comparative results when employed under suitable conditions.

Osborne's application of the Hausmann method to his preparations marks the beginning of a new era in the problem of protein characterization. He wrote (56), "we have found by its use that some of our preparations from different seeds which were so nearly alike in composition and reaction that no difference could be detected between them sufficient to warrant the conclusion that they were not the same chemical individual, yield such different proportions of nitrogen in the several forms of binding that there can be no longer any doubt that they are distinctly different substances. On the other hand, many preparations of different origin, which we have heretofore considered to be identical, have yielded the same proportion of the different forms of nitrogen and consequently our former opinion respecting the identity of these protein preparations is very greatly strengthened."

Judged by the new criterion the edestins derived from hemp and cotton seeds and the castor bean still appeared closely alike but differed from those from other sources. These others likewise differed among themselves and, consequently, instead of a single edestin derived from ten different seeds there were now at least seven different proteins that had previously been referred to under this one name. Cotton seed globulin had been found to react positively towards the Molisch reagent while hemp seed and castor bean globulins did not. The name edestin was therefore restricted to these two globulins with the further cautious statement (56): "Whether the globulins from these two seeds are in fact alike is rendered doubtful by the other

results of this investigation, for only those proteins appear to be identical that originate from seeds which are closely related botanically." The legumins obtained from pea, lentil, horse bean and vetch gave closely agreeing results and were therefore still held to be identical with each other as were the legumelins from the same seeds. The corylin of the filbert differed from the similar protein from the English walnut with which it had previously been identified. The gliadins from wheat and rye still seemed identical, but hordein from barley was slightly different and zein from maize was widely different from these. Thus in one clean-cut investigation Osborne showed that most of the vegetable proteins with which he was familiar differed more or less among themselves. Although the notion of absolute specificity could not come until a method of a finer power of discrimination, the later developed anaphylaxis test, was used, it was clear that purely chemical methods were capable of yielding highly important results.

Studies were undertaken of the tryptophane reaction, the Molisch reaction, the solubility limits in salt solutions and of the specific rotation of different proteins. Beginning in 1906, with the aid of a number of collaborators, Osborne carried out a series of analyses of the amino acid composition of proteins employing Kossel's method for the basic amino acids and Fischer's ester distillation method for the mono amino acids. These studies set a standard for such work which has been surpassed only since the introduction, in recent years, of greatly improved methods for dealing with certain of the amino acids. Characteristically, he returned again and again to the analysis of a few of the proteins, such as casein, gliadin, and zein, which possess special economic importance, each time increasing the summation of the components by the use of more refined technique.

By 1908, when the paper on "The Different Forms of Nitrogen in Proteins" (96) appeared, data had been accumulated clearly indicating that most of the known proteins could be satisfactorily characterized by the methods of amino acid analysis, coupled with a study of the physical properties. In a review

of his own work (94) published at this time Osborne stated: "In considering the position of our present knowledge of the seed proteins, the question of chemical individuality should first be considered. We are now well past the time when agreement in solubility, ultimate composition and color reactions, are to be accepted as evidence of the identity of two preparations of protein. It is not necessary to explain why it is at present not possible to demonstrate the chemical individuality of any single protein, for the reasons are evident to all who will give this question the slightest consideration from the standpoint of the organic chemist. While it is not possible to establish the individuality of any protein, it is possible to show differences between the various forms which can be isolated, and to establish a constancy of properties and ultimate composition between successive fractional precipitations which give no reason for believing the substance to be a mixture of two or more individuals.

"On the basis that agreement in ultimate composition affords no evidence of identity of two similar proteins, but that distinct and constant differences in composition are conclusive evidence that they are not alike, I have endeavored to differentiate the several seed proteins that I have studied, and have since subjected them to careful comparisons in respect to their physical properties and the proportion of their decomposition products, so that those which are alike in their more apparent characters have been still further distinguished from one another. Whether these are in fact chemical individuals, must await the development of new methods of study. For the present they must be accepted as the simplest units with which we can deal."

At the present day there is probably no qualified protein investigator who would be prepared to assert that any two proteins from different sources are in all respects identical. To be sure there are a few pairs or small groups of proteins, notably those from different varieties of maize or the gliadins from wheat and from rye, the differences between which are no more notable than the differences between two preparations of the same protein. Even the extremely delicate biological tests for

differences sometimes fail to discriminate between them; but to argue from this that no difference at all is present implies that more is known of these proteins than is really the case. Largely as a result of Osborne's careful characterizations of the seed proteins it is now generally held that each kind of plant and animal cell has its own equipment of specific proteins. While similarities occur in many cases and, in fact, are sometimes very conspicuous, rigid identification between proteins of unlike origin is not at present possible.

The demonstration that proteins differ widely in their amino acid composition and the definite knowledge of this composition that had been obtained, turned Osborne's attention in 1908 back to a problem that had been foreshadowed in some of his early papers and which he had had in mind almost from the beginning. In 1902 (56) he had written, "The animal can . . . synthesize protein from a mixture of the crystallizable products produced by the decomposition of proteins. Since such a wide difference exists between the proportions in which the several groups of products are yielded by the different food proteins, this synthesis must consist in something more than a recombination of the several fractions of the molecule of the food protein; it must involve a more or less extensive alteration of these fractions and conversion of one into another before the requisite number of groups of proper nature are at hand from which the new molecule can be constructed." He had realized, however, that until pure and uniform material could be obtained in abundance and its composition established by chemical analysis, an investigation of the comparative nutritive properties of proteins was useless. The striking differences which now became evident in the composition of many of the proteins suggested that their biological values might be correspondingly unlike.

In 1909 this third phase of the work was begun in collaboration with Professor Lafayette B. Mendel of Yale University; their joint labors continued without interruption until Osborne retired in 1928. The investigation of the nutritive properties of the proteins involved the development of a technique for feeding individual small animals which would permit accurate measure-

ments of the food intake. This was successfully accomplished, but the first experiments in which the pure isolated proteins were fed, together with sugar, starch, lard, and an inorganic salt mixture, showed that normal growth of young animals did not take place, although mature animals, as well as young, could be maintained for considerable periods. Growth of young animals could readily be secured when dried whole milk powder was furnished together with starch and lard. This appeared to indicate that milk contained something essential for growth other than protein. The preliminary assumption was made that the missing factor might be supplied by the inorganic constituents of the milk, and it was indeed soon found that excellent growth could be secured when evaporated milk serum from which casein and lactalbumin had been removed, the so-called "protein-free milk", was added in sufficient amounts to a diet of isolated protein, starch and lard. With the assistance of this material an extensive investigation revealed wide differences in the alimentation of animals on different proteins. Animals rapidly failed on zein and gelatin, were maintained at constant weight on hordein, rye and wheat gliadin, but grew well on edestin, wheat glutenin, lactalbumin or casein (114, 115). Further work showed that the failure of animals on a zein diet was due to the lack of the amino acids tryptophane and lysine in this protein; when these were supplied growth occurred (130). Similarly, gliadin could be made adequate for growth by an addition of lysine in which this protein was conspicuously deficient (141).

The use of protein-free milk in diets was attended by certain difficulties. It was not entirely free from nitrogen and it could not be successfully replaced by an artificial mixture of salts made to imitate the composition of milk ash as closely as possible. Furthermore, animals nourished on this diet over long periods ultimately ceased to grow and declined rapidly in weight. In every case such animals could be brought to a normal rate of growth by changing to a diet that contained whole milk powder, and the ultimate failure on protein-free milk could be postponed or averted by feeding whole milk powder for occasional short

intervals. An examination of the composition of the two types of food revealed that the most conspicuous difference lay in the presence of milk fat in the dried milk food. Experiment soon showed that the addition of butter to a casein, starch and protein-free milk diet sufficed to permit normal growth to maturity. When butter was added to a diet of dried skim milk upon which it had been found that animals eventually failed, complete realimentation occurred.

These results were published in 1913 (134). The paper describing them was submitted to the *Journal of Biological Chemistry* about three weeks after a paper by McCollum and Davis in which similar results, secured by the use of an ether extract of egg yolk and of butter, were given. The observations indicated that some substance occurs in butter which is essential for animal growth. This substance was later designated as vitamin A.

In the following year the important observation was made (142) that the same stimulation of growth could be secured by the addition of cod liver oil to a diet of purified food substances and protein-free milk, a discovery which served to focus attention upon the value of this oil, in particular as a curative agent for the peculiar eye condition known as xerophthalmia that was regularly encountered by Osborne and Mendel in animals on the deficient diets. At the close of the war the sight of many children in Europe was preserved by its use, a remarkable example of the application of scientific results to practical problems.

The later extensive contributions of Osborne and Mendel and their associates to the science of nutrition can only be indicated. Much labor was devoted to the study of the nutritive value of the proteins of the commercially important foods and this work gave a rational explanation of many practices that empirical experience had shown to be advantageous. The distribution of vitamins in natural food products was studied and considerable success was attained in an effort to prepare a vitamin B rich concentrate from yeast. The phenomena of growth, its suppression and acceleration under various regimens,

the effect of the individual inorganic constituents of the diet, these and many other topics received attention at different times.

The remarkable influence of minute traces of certain organic substances, the presence or absence of which in the diet determine success or failure of nutrition, drew attention to the importance of an investigation of the constituents of living cells. This led to a detailed study of extracts of the alfalfa plant and of yeast, both of which are valuable sources of vitamins. Much of the information secured did not reach the stage of publication, but a striking demonstration was obtained of the complexity of the chemical environment in which the life of the cell takes place.

It would be incorrect to assume that Osborne's interest in the fundamental chemistry of proteins waned as he penetrated more deeply into the mysteries of animal nutrition. Innumerable chemical problems arose as a result of the feeding work and demanded solution. Such, for example, was the discovery in 1913 of lysine among the products of hydrolysis of gliadin (132): its presence had escaped the notice of previous observers, including himself. A study of the constituents of milk in 1917 revealed a new protein soluble in dilute alcohol (180), the first animal protein that possessed this property to be found. Its anaphylactogenic relationships were worked out in collaboration with Professor H. Gideon Wells in 1921 (224) and it was demonstrated to be distinct from the other three proteins of milk.

The division of the present discussion of Osborne's work into three parts which correspond to the periods when he was chiefly interested in the preparation, the analysis, and in the biological properties of proteins has involved an omission of reference to several lines of investigation that bore less directly on the main theme.

In 1895 an investigation (26, 27, 39) was made of the chemical nature of the amylolytic enzyme diastase from barley malt. This study was undertaken on account of the similarity in the chemical properties of the albumins prepared from wheat, rye, and barley and because of the observation that extracts

from these seeds possess diastatic properties. By the application of the methods employed in protein investigation, specimens of an albumin-like protein were secured from malt with more than six times the amylolytic power of the most active preparations of diastase that had previously been described. It was shown that the enzyme had all of the properties of a protein while, at the same time, the method of preparation excluded the possibility that appreciable amounts of non-protein substances were present. The most active preparation produced ten thousand times its own weight of maltose from starch in seventeen hours at room temperature and it must therefore be reckoned, even today, as an extraordinarily active material. The necessity that chlorides be present in order that diastase may exert its maximum effect was pointed out and many observations were made of the effect of other salts on diastatic activity. These papers were the first that clearly proved the protein nature of an enzyme, they laid the foundation for the subsequent work of others on the enzymes that bring about the digestion of starch to sugar and they have had a far-reaching effect upon the development of the whole problem of the chemical nature of these substances.

In 1900 Osborne made a fundamental contribution to the chemistry of nucleic acids when he announced the discovery of tritico nucleic acid in the wheat embryo and observed that this substance yielded the purines, guanine and adenine, in molecular proportions. He made it clear that the various nucleoproteins that could be prepared from the wheat embryo were in reality salt-like compounds of one and the same protein with variable proportions of nucleic acid; generalizing from these observations he pointed out that the numerous nucleoproteins from animal sources that had been described were, very probably, also salt-like compounds of protein with nucleic acid.

A paper on the proteins of the castor bean published in 1905 (69) is of interest since it marked Osborne's first collaboration with Mendel and also illustrates the remarkable refinement of his methods of protein isolation. The castor bean contains an albumin, ricin, and a globulin, together with proteoses of less

clearly defined properties. Ricin is the best known of the toxalbumins. These are proteins and are among the most extraordinarily potent poisonous substances that have ever been described. Osborne made preparations of this material that were fatal to rabbits in doses of 0.0005 mg. per kilo of body weight, that is, one part of the preparation killed two thousand million parts of rabbit. Ricin itself was not secured in a form entirely free from protease, but it was separated from the globulin that accompanies it in the seed so completely that no physiological effect was observed when large doses of the globulin were administered to animals. The hemagglutinin in the castor bean was found to be identical with ricin and the protein nature of this toxalbumin was thoroughly established.

All of the preparation work and much of the chemical investigation of the vegetable proteins were completed before the present day conceptions of acidity had been advanced, nevertheless Osborne had noted the effects of different degrees of acidity on these substances and was alive to the significance of the phenomena. One of his early papers on edestin contains the phrase "the concentration of the hydrogen ions in the solution," and it was his custom invariably to state the indicator which he used when adjusting the reaction. It was not sufficient to neutralize a solution; the solution was neutralized to phenolphthalein, or litmus, or tropeolin, as the case might be, and the differences in behavior observed were fully appreciated. It is this careful attention to detail which gives Osborne's early work a value to the physical chemist and renders it possible to furnish interpretations in terms of modern theory, as has recently been done by Cohn.

The problem of protein differentiation was largely solved when the methods of amino acid analysis were applied to the proteins, but several pairs or small groups of proteins remained that were still indistinguishable by purely chemical means. In 1911 Osborne collaborated with Wells in a study of the anaphylaxis reaction of vegetable proteins that was continued for several years (111, 131, 139, 140, 151, 163). This was the first serious investigation of the biological reactions of such pro-

teins and it led to many valuable conclusions. A number of differentiations were made between proteins that had, up to this time, revealed no chemical differences. Thus the globulins juglansin and corylin from the American black walnut and the filbert, respectively, were found to be distinct as were the flax seed, cotton seed and hemp seed globulins, and the phaseolins of kidney bean and adzuki bean. On the other hand the gliadins of wheat and of rye and the legumins of pea and of vetch reacted strongly with each other. No chemical grounds for the differentiation of these proteins had been found. This result invited the final conclusion that the respective pairs of proteins were really identical. Such a conclusion could not safely be drawn, however, because a number of cases were encountered in which chemically distinct proteins gave powerful anaphylactic reactions with each other. Thus the gliadin and glutenin of wheat reacted positively with each other as did gliadin from wheat and hordein from barley and the vicilin from pea with the legumin from vetch. These reactions were attributed to the existence of common reactive chemical groups in the respective pairs of proteins. The specificity of the anaphylaxis reaction was therefore held to depend on the chemical structure of the protein molecule, but structures sufficiently alike to give rise to this reaction are only observed in proteins derived from plants that are botanically closely related.

The first public recognition of Osborne's exhaustive work came from Germany. V. Griessmayer, in 1897, published a translation of Osborne's papers on vegetable proteins in book form and stated in the introduction that it was his object "to bring to light these treasures buried in the American publications." This encouragement came at a time when few of his associates or scientific friends had any conception of what his work meant. In 1900 he was awarded a gold medal by the Paris Exposition. In 1910 recognition came from Yale University in the form of an honorary degree of Doctor of Science; and in the same year he was elected a member of the National Academy of Sciences and to the presidency of the American Society of Biological Chemists; two years later he was made an honorary

fellow of The Chemical Society (London). In 1914 he was made a fellow of the American Academy of Arts and Sciences and in 1921 was elected to the American Philosophical Society; in 1922 he received the John Scott medal and in the following year was made a research associate in biochemistry of Yale University with full professorial rank. In 1928 he was the first to receive the Thomas Burr Osborne gold medal founded by the American Association of Cereal Chemists in recognition of his outstanding contributions to cereal chemistry.

Osborne's extensive investigations would have been impossible without generous financial support. Throughout the early years, when results came slowly and their application was by no means apparent, the directors of the Connecticut Agricultural Experiment Station, at first Professor S. W. Johnson, and after 1900, Dr. E. H. Jenkins, with the cooperation of an enlightened board of control, allowed no interference or distraction to hinder the progress of the work. After 1904 a large proportion of the financial burden was borne by the Carnegie Institution of Washington, D. C., of which he was a research associate. His connections with both the experiment station and the Carnegie Institution of Washington furnish a striking example of the value to science of a policy of non-interference on the part of those in control of the distribution of funds for research. Except for routine annual reports he was never asked for statements of progress nor for outlines of projects. The relationship was always one of the utmost mutual confidence and esteem.

The results of many of his investigations were summarized in a monograph, "The Vegetable Proteins," which first appeared in 1909 (101) and was thoroughly revised in 1924 (243). This small volume has become the classical publication in the field. His extensive studies of wheat proteins were reviewed in 1907 in "The Proteins of the Wheat Kernel" (80), now a standard book of reference among cereal chemists. Including these and a few public addresses and popular articles a complete bibliography of his publications reaches 252 titles, of which about two hundred are journal reports of his personal scientific work.

To those who were privileged to be associated with him Os-

borne was a rare stimulus, a formidable opponent in argument and an ever genial but just critic. He frequently closed a discussion with the remark that facts were to be found in the laboratory, not in books. Naturally shy and retiring, the delivery of a public address or of a paper was a severe trial to which he looked forward with trepidation, but among a small group of friends he showed himself as a gifted conversationalist, who was equally able to discuss the latest achievements of science, the current political situation, the intricacies of the world of finance or the faults of the modern educational system. His most marked characteristics were, perhaps, the thoroughness with which his problems were investigated and the caution with which his results were presented to the scientific public. In the early preparation work each protein was isolated in as many different ways as possible, the composition finally ascribed to it was deduced from a large number of carefully conducted analyses and, where the economic importance of the protein warranted it, he returned again and again to its study; casein and the wheat and maize prolamins received extraordinary attention. Time and again he discarded painfully acquired results to make a fresh start, this time to "do it right," as he expressed it. His publications, which cover some four thousand printed pages, are marked by lucidity of style, directness of statement and freedom from self-conscious literary adornment. All that he wrote bears the marks of careful editing lest a statement should to the slightest extent pass the bounds of ascertained fact.

He was more fortunate than most men in that advancing years, distinctions and scientific recognition did not bring with them administrative responsibilities which deprived him of the opportunity to share in the daily work of the laboratory. His time was always freely available for discussion, not only with his associates, but with the innumerable investigators from all parts of the world who came to New Haven to see him and ask for advice. Ever kindly and courteous, with keen insight into the problems of others and an extraordinary wealth of experience upon which to form his judgments, he has left a memory that will long be treasured by those who had the privilege of knowing him.

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